SCIENCE

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE METHOD OF SCIENCE[1]

SCIENCE governs human life by determining the conditions of existence and by furnishing the means of civilization. Religion prescribes the motives, government formulates the customs of mankind, science fixes what we can do and how. If, at the present meeting, we appropriately emphasize the rôle of science, it does not imply that we belittle the ethical or social factors of civilized life, but answers the demand for a more just and general recognition of the actual importance of pure science.

We are so accustomed to the practical advantages that have followed from abstruse science, that we connect them with their source only by a distinct mental effort. The wonders of practical science have been recited so often, that their reiteration has become tedious, and we no longer feel strongly impelled to felicitate mankind on the parlor match, the telephone and the antitoxines, although we indulge at present in an unsubdued excited anticipation of wonders to come, especially in the domain of medicine. Are we not all on the watch for the announcement of the cure for cancer, and vaguely for other new and astounding reliefs from disease! Such concentration of interest upon novel practical results is not wholly favorable to science.

It is true that a large amount of investi-

[1] Vice-presidential address delivered before the Section of Physiology and Experimental Medicine of the American Association for the Advancement of Science, at Minneapolis, December 29, 1910.

gation is going on which aims to secure immediate practical results. In chemistry and medicine especially the activity in the work of applied science is very great. This condition gives a powerful fresh reason for defending pure abstruse science. Applied science always has been, is now, and probably always will be distinctly subsidiary to pure science. The final justification of all scientific research is undoubtedly the power it creates for the use of mankind, but the power must be created before it can be used. A little study of the history of science should suffice to convince any reasonable mind that the command we possess to-day over nature is due to the labors of men, who have almost invariably pursued knowledge with a pure devotion uncontaminated by any worship of usefulness. These devoted idealists have gathered the varied mighty harvests by which all men have profited, but the debt of gratitude to them is unpaid.

The pursuit of abstruse science needs to be encouraged. It is insufficiently esteemed. This doctrine ought to be emphasized on all suitable occasions, but especially before the section of experimental medicine. The people cry for relief from sickness and their demand for prompt useful discoveries is so urgent that there is danger in it, since it tempts medical investigators away from the fundamental enquiries, which, answered, will give great results, and seduces them to work exclusively at secondary problems, from the solution of which quicker, but smaller results may be expected. Pure science is broad; it embraces all. Applied science is a congery of fragments, of isolated problems, which lack cohesion and are without any necessary connection with one another. It is easy to understand why students of applied science have seldom made great discoveries.

In fact, scientific knowledge will not be compelled. We have to take what knowledge we can get, and by no means can we get always what knowledge we want. Pure science adapts its undertakings to these rigid conditions, and works where the opportunity is best—not so applied science.

Let us recall a few of the epoch-making discoveries. When Galileo turned his lean face up towards the swinging lamp in the Cathedral of Pisa and as he looked discovered the law of the moving pendulum, he was in quest of pure knowledge. We can not conceive such a man actuated by any lower motive. Even when we learn of his astonishing the Venetian merchants by enabling them to see their far-off vessels through his newly invented telescope, do we not feel that it was merely an episode to Galileo? Such a man does not ask "What use is it?" His demand for knowledge was insatiable. When Newton thought out the problem of gravity and his theory of planetary motion; when Malpighi explored the structure of animals with his crude microscope; when Lavoisier created modern chemistry; when Cuvier combined comparative anatomy and paleontology and made the combination yield new revelations; when Lyell proved geological history to be an evolution and not a succession of cataclysms; when von Baer against immense difficulties traced the development of the chick; when Schwann demonstrated the correspondence of cellular structure in animals with that of plants —was one of them actuated primarily by the wish to get practical results? We have only to read their works to convince ourselves that they were all in search of knowledge for knowledge's sake. Yet they are the giants of human history, who in importance are approached by few monarchs or statesmen. Compared with the growth of science the shiftings of govern-

ments are minor events. Until it is clearly realized that the gravest crime of the French revolution was not the execution of the king, but the execution of Lavoisier, there is no right measure of values, for Lavoisier was one of the three or four greatest men France has produced.

Since pure science has been preeminent in the past not only in furnishing useful knowledge, but also as a chief foundation of human progress, and is likely to long continue equally preeminent, it is well worth while to study the general principles by which original research is guided. No previous definite study of these principles is known to me, although I have searched not a little to find one. All that I have been able to discover are treatises on logic, the reading of which, most active investigators would, I fear, find tedious and unprofitable rather than helpful and inspiring. We have too many real difficulties to quite enjoy wading through the artificial morass of pedantries, in which logicians by profession embed their significant truths. The stricture is severe, but not too severe even for so sound and valuable a work as Jevons's "Principles of Science." It must be doubted very seriously whether the study of logic is really essential for the right training of an investigator. While it goes without saying that logical thinking is indispensable in science, neither may it be overlooked that thinking is a complicated physiological function, which is brought to efficiency by practise, and that training by actual use is the one indispensable means of disciplining and developing the function. Playing the violin is a complicated physiological function, but it is not thought necessary that the violinist should study the anatomy of the muscles and nerves of the hand and arm. He perfects himself by practise. Anatomical knowledge might enable him to understand why he can make certain motions and can not make others. Our analogy limps perhaps, but is a real analogy, for practise in right thinking creates the necessary habit of being logical, and ability to describe the mental processes in the language of logicians is an accomplishment which few even of the greater scientific discoverers possess.

It is my belief that the logical work of scientific men is usually well done, and is the part of their work which is least faulty. The difficulties and the majority of failures are due, it seems to me, to two chief causes, the first inadequate determination of the premises, the second exaggerated confidence in the conclusions. If I am right, the method of science is the result of the effort to get rid of these two causes of error.

We must recognize in starting that the expression "the method of science" means more than "logic," being far more comprehensive when rightly defined. We can not alter the fundamental conditions of knowledge, for we are still unable to add new senses or improve the brain—although eugenics dreams of a future with such possibilities—nor can we change the nature of the phenomena. The same fundamental resources are available for daily life and for science. We must be clear in our minds on this point, in order to comprehend that the fundamental distinction of the scientific method is its accuracy. As I have said on another occasion "there is nothing to distinguish the scientific method from the methods of every-day life except its precision. It is not a difference in kind or quality, but a quantitative difference, which marks the work of the true scientist and gives its validity." Such being the case, a broad examination of the method of science reduces itself to the

study of the general principles of securing accuracy.

If you will examine frankly your own opinions and those of your acquaintance you will, it may be presumed, quickly acknowledge that many, perhaps most, of the opinions are not of scientific accuracy. On the contrary, they are, to a large extent, mental habits and the result of the summation and averaging of impressions. I, for example, know a generous man, but can give very little of the evidence on which my opinion is based. I know a seacoast on which fog occurs in summer quite frequently, yet I can not state how often the fog occurs nor just when I have observed it. At sundry times I have received an impression, in one case of the man's generosity, in the other of fog. The exact data can not be recalled, but the impression on my mind has been fixed by repetition. The evidence is lost, but the conclusions persist and are accepted by me as correct. For my practical needs they are sufficient. We get along in ordinary life satisfactorily enough with opinions thus formed by summation. Most human opinions, even when they are merely imitative, originate in this way, and are correspondingly unreliable. If we seek to explain the fallibility of ordinary opinions and testimony must we not attribute it to the absence of the detailed evidence and the consequent impossibility of verifying the testimony?

We are thus led to recognize the preservation of the evidence as the fundamental characteristic of scientific work, by which it differs radically from the practise of ordinary life. I venture accordingly to define the method of science as the art of making durable trustworthy records of natural phenomena. The definition may seem at first narrow and insufficient, but I hope to convince you that it is so comprehensive as to be not only adequate, but also almost complete.

All science is constructed out of the personal knowledge of individual men. Science is merely the collated record of what single individuals have discovered. Accordingly, we must consider, first, the way in which the individual knowledges are recorded and collated. The process begins, of course, with the publications of the special scientific memoir in which the investigator records his original observations and makes known his conclusions. Permit me to quote from Oldenburg's preface to the first volume of the *Philosophical Transactions of the Royal Society*. The date is 1665.

> Whereas there is nothing more necessary for promoting the improvements of Philosophical Matters, than the communicating to such, as apply their Studies and Endeavours that way, such things as are discovered or put in practice by others; it is therefore thought fit to employ the *Press*, as the most proper way to gratifie those, whose engagement in such Studies, and delight in the advancement of Learning and profitable Discoveries, doth entitle them to the knowledge of what this Kingdom, or other parts of the World, do, from time to time, afford as well of the progress of the Studies, Labours and attempts of the Curious and Learned in things of this kind, as of their compleat Discoveries and performances.

All that he says is true to-day, although our taste has changed in favor of shorter sentences.

It is interesting to note that our present standards for original memoirs have developed gradually. In Harvey's essay on the circulation of the blood, published in 1628, there are no precise data as to his observations. The author does not think it necessary to specify how he has laid bare the heart or how often he has repeated his observations. His descriptions of the beating heart are vividly realistic. He writes with conviction and authority. The reader

is compelled to believe him. Harvey, however, does not provide information to facilitate repetition of his work—he offers little aid towards the verification of his results. Francesco Redi, the founder of experimental biology, published his "Generation of Insects" in 1660. His experiments proved that insects are not spontaneously generated in putrifying meat. His conclusion[2] is sound, but he does not give more than a general account of the actual experiments. A century later Spallanzani established the modern standard, and in his works we find the details as to his evidence put down with scrupulous care, for example in his paper on the circulation (1773) the single experiments are exactly described. But Spallanzani in this, as in other respects, was far in advance of his time.

In a cotemporary article we expect a presentation of all the data necessary to render subsequent verification by other observers possible. We further expect clear information as to the amount of material on which the observations were made, or the number of experiments on which the work is based. In other words, a modern investigator will hardly receive consideration for his researches unless he furnishes every aid he can to facilitate criticizing and testing his results. This severe standard has been only gradually evolved, but is now stringently enforced in all departments of science and is the response in our practise to our need of eliminating the purely personal factor. It would be advantageous if scientific authors generally viewed the obligation of providing for verification as an even more serious duty than it is esteemed at present. It might, indeed, be a wholesome practise to demand that every scientific article should contain a special section or paragraph on the means of verifying the result, for verification by *Fachgenossen* is second in importance only to discovery in the progress of science.

The conditions of scientific progress have changed greatly though very gradually. Two hundred years ago the number of active investigators was small. This year there are at least ten thousand men of substantial ability carrying on original researches, consequently each theme is being worked at by several men, and the final outcome is the consequence of collaboration, which is none the less actual and effectual because it is unorganized, and is usually not formally designated as collaboration. For example, our present knowledge of the complex and very varied processes of cell-division has been constructed not merely by successive accumulations, but also by incessant debate and repeated mutual criticism. If we examine a paper on mitosis we find not merely the author's own observations, but also references to other related investigations, to specify which there is often a formidable bibliography. Within a generation the modern science of bacteriology has been created. Within a few years radiology, the wonders of which still thrill us, has suddenly come into existence. Both great achievements are the results of both the original observations and also the constant mutual discussions of a number of scientific men.

These conditions have rendered great men somewhat less important than formerly. Science grows by the accretion of ideas. Now, a great man has, let us say, twelve new ideas, where a man of ability has one. If science gets twelve new ideas it matters little whether they come from one man or from twelve. To a certain

[2] At vero ubi loco ita clauso illud (stercorem bovis) dentinui, ut intrare muscae & culices, et ova sua ponere non possent, nihil omnino natum vidi.

extent numbers make a substitute for genius—but nothing probably will ever replace that type of great genius, to which we owe most, the man who has a great thought, which no one has ever conceived before.

The nineteenth century in response to the new conditions, which have arisen in its course, has added another new standard for scientific memoirs—they must include a conscientious consideration of recent and cotemporary related work. Now the second step in science-making, after recording the new original observations, so as to make them accessible to others, is the collation of these same observations into broad general results. The aim is to eliminate the personal factor and to impart the character of impersonal absolute validity to the conclusions.

In addition to the original memoirs science profits by a large number of publications, almost all of which are of modern, often of very recent, creation. Broadly speaking, their aim is to promote that collation, which is begun in the original memoirs. Germany is the home of most of these undertakings, which are familiar to us under the names of "Jahresberichte," "Centralblätter" and "Ergebnisse." So far as I have learned, Jacob Berzelius's "Jahresberichte" for the physical sciences, which Gmelin translated into German, issuing the first volume at Tübingen in 1822, was the first ancestor, the Adam, of this modern biblic race, which therefore can not yet celebrate its first centenary. As concerns those branches of biology known as the medical sciences, the summarizing publications under consideration have become important only since 1870, although they began earlier. For biology 1834 may be taken as the starting point, for it was the initial year of Schmidt's "Jahrbücher der gesammten Medicin" and of Johannes Müller's first Jahresbericht. Meckel had just died and Müller assumed the editorship of the *Archiv für Anatomie und Physiologie,* which he conducted for so long that it is still often known simply as Müller's *Archiv,* although the *Archiv* since his death has had several distinguished editors. Müller wrote for the *Archiv* the first Jahresbericht entirely himself. His report is interlarded with many keen criticisms and even with references to unpublished observations of his own. Later he engaged others to assist in the yearly reports, which were kept up until 1857. Their place was taken by Henle's Jahresberichte, which were continued until 1871, when they in turn were replaced by the *Jahresberichte der Anatomie und Physiologie* founded by Franz Hoffmann and Gustav Schwalbe in 1872. The growth of anatomical science is indicated by the fact that in round numbers 400 pages sufficed for the abstracts of anatomical papers in 1872, but 1,500 were necessary in 1908. Similar increases have occurred in the output of the other medical sciences, hence it has become more and more difficult to bring out the Jahresberichte promptly—a delay of two or three years is common. To meet this growing difficulty the various Centralblätter have been started—those with which we are here concerned are periodicals issuing small numbers (Hefte) at short intervals and filled with brief abstracts of recently published researches.[3] They have proved of limited utility and their completed volumes are so inconvenient to consult that one

[3] The dates when some of the Centralblätter started are as follows: für medizinische Wissenschaften, 1863; für Physiologie, 1887; für Bakteriologie, 1887; für allgemeine Pathologie, 1890; für allgemeine Biologie, 1910. Although the number of German "Centralblätter" is very large, yet in other countries corresponding magazines are viewed with limited favor.

habitually avoids them. They are useful, perhaps, at the moment of publication, but the back volumes encumber rather than enrich our libraries. Fortunately the last decade of the nineteenth century brought us a new and very valuable form of report, the avowed purpose of which is the systematic collation of results. I refer to the "Ergebnisse." The earliest of them known to me was founded in 1892 by Merkel and Bonnet to cover anatomy and embryology. The annual volumes contain essays on various topics which really collate recent discoveries; they differ fundamentally and advantageously in method from the Jahresberichte and Centralblätter, by presenting a combined picture rather than abstracts of single papers. They are substantial contributions to science because they systematize and coordinate the new information. The enterprise of Merkel and Bonnet deserves our most grateful appreciation. Its value is witnessed to by the foundation of similar "Ergebnisse" for other sciences. The series for pathology began in 1896, for physiology in 1902, for zoology in 1909. In the admirable *Revue d'Histologie* (1906) they found a French follower. The "Ergebnisse" are likely to prove of increasing importance and as the number of new investigations mounts higher and higher their comprehensive essays will become even more indispensable than at present.

Although logically more remote from the original sources than the annual and special collations just reviewed, yet handbooks are historically older. Formerly one man could master completely his whole science and keep up with all the new discoveries. In 1834 Johannes Müller wrote the whole annual report upon anatomy, comparative anatomy and physiology, and did it well. A hundred years ago a single author could write a thorough manual. To-day such a feat is impossible. The difficulty has been met with commendable success by cooperation. A science is divided into chapters; each chapter is undertaken by a specialist, and so the task is done, but with consequences easily anticipated, for every one of us knows some of these huge modern composite hand-books.

We recognize in the present methods of recording and collating scientific discoveries many adaptations which are due, it seems to me, essentially to the mere increase in the number of workers. But though the methods are modified the essential steps are the same: *first,* the record of the individual personal knowledge; *second,* the conversion of the personal knowledge by verification and collation into valid impersonal knowledge; *third,* the systematic coordination and condensation of the conclusions.

A defect—perhaps the most serious defect of our education—arises from our failure to make our students appreciate vividly the fundamental fact that science is based on personal knowledge. Our students are allowed to graduate from college, for the most part without any comprehension of this great truth. The best of them start forth with a high reverence for the library, the place of records, but quite unaware that a still higher reverence is due to those who, by being the first to observe unknown things, have founded the knowledge, the records of which the library keeps.

The divergence between philosophy and science shows itself most conspicuously in the personal mental attitude, which philosophy cherishes and science seeks to overcome. Philosophers still discuss philosophers and their systems, scientific men pursue impersonal knowledge with such

ardor that they are apt to know little of the history of science.

May I venture to divert your attention to two matters, which suggest themselves in connection with our main theme? The first is the question of style in original scientific articles, for we probably all are ready to admit that the care bestowed on the presentation in print and picture of original discoveries is often insufficient. Do we not all know articles which are bungled in form and weakened by prolixity? Surely the heads of laboratories should insist by example and precept that all the workers under their influence should write clearly and briefly—for if an author fails to show respect for his own scientific work, how can he expect others to respect it? Yet there are few matters so important as intensifying the world's respect for science. For us, whose language is English, the standard should be the highest. Rivarol in his famous prize essay said "ce que n'est pas clair, n'est pas Français"—but we might say what is not true, is not English. By its wealth of synonyms and its logical construction the English language is preeminently adapted to the exact statement of scientific truth. We should not misuse so fine an instrument, which if well employed is sure to win for Anglo-Saxon science the wide influence it deserves. Good thinking is the blastema of good style, therefore our learning will never appear good if our learned articles are written badly.

The second matter for digression is a suggestion concerning bibliography. Almost every important memoir is accompanied by a bibliography. Custom prescribes it. The literature is indicated by the titles in full, and when the list is well made the volume, page and plates are all given. Other memoirs on the same subject give similar bibliographies. We know from experience that these selected bibliographies are very helpful to those who follow—but is there not a needless waste through frequent repetition? There would be a great economy if we had a complete international catalogue of the scientific literature of each year, in which all the publications of each author were entered with serial numbers. It would then suffice to quote an author's name, the year and the serial number, as, for example "John Doe, 1910, 1," to give a complete reference, for it is to be presumed that the catalogue would be found in at least all the principal scientific centers of the world. This system has been utilized privately already, and experience with it has demonstrated its eminent practicability and simplicity in use. The International Catalogue of the Royal Society, which is at present not only imperfect but excessively inconvenient and really of little use, might be transformed by the plan suggested into an invaluable aid to science. The plan could be still more easily applied to the cards of the Concilium Bibliographicum of Zürich. It is deplorable that the Royal Society neither cooperates with, nor adopts the system of, the Concilium. As matters are the International Catalogue remains merely a respectable failure.

To return: The records, which we have considered thus far are those which serve to make the discoveries of individuals available for others. As soon as the discoveries are properly collated and sufficiently verified they become permanent parts of science. Many definitions of science have been given, and did time permit it might be profitable to quote some of them—but is it not sufficient to define science as *knowledge which has acquired impersonal validity?*

We must now attempt a general examination of the records, which are used pri-

marily to help the original investigator, though often preserved to assist his successors. The simplest form of record is the preservation of the actual specimen. Scientific museums are essentially storehouses for such records. Most of them to be sure maintain public exhibitions, which interest, stimulate and possibly instruct the public, but the precious part of their collections comprises the objects possessed, which have served for some original discovery. Scientific museums are very modern, nearly all those in America have been started within a few years. The Philadelphia Academy of Natural Sciences was founded in 1812, the Boston Society of Natural History in 1831, Agassiz's Museum in 1859, the National Museum in Washington in 1876,[4] and the Field Columbian Museum in 1893. A history of museums, dealing especially with the progress of the art of caring for collections would be cheering to read, for it would picture a remarkable growth of the appreciation of the value of objects as original records. This may be illustrated by the change of opinion as to "type" specimens of plants and animals. The systematic zoologists and botanists constantly lament that the earlier authors did not preserve the actual specimens from which they described new species and they consider no pains too great to ensure the preservation of "types" of new species, which any cotemporary worker describes. In the Laboratory of Comparative Anatomy at Harvard we have felt the influence of the example of museums and have established a permanent embryological research collection, a sign of the times and an acknowledgment of the new insistence upon the preservation of the original proofs of discoveries.

[4] The genesis of .this museum dates back to Smithson's bequest, 1826, and was in part due to accumulations of materials from various government expeditions before 1876.

The progress of science is marked by the advance in the art of making research records. We all admit, in other words, that the progress of science depends partly on the perfecting of old methods, but chiefly on the invention of new ones. Despite the enormous variety in their nature and aims, all our technical methods have this in common that their real purpose is to yield us records. Our microscopes, spectroscopes, measuring instruments and many another apparatus have indeed their primary scope in rendering possible observations, which are impossible with our unaided senses. They enlarge our field of enquiry and put precision within our reach. Yet their usefulness is conditioned upon their enabling us to make records which else would remain beyond our power. On the other hand, there is a still larger class of apparatus which are obviously designed to make records. What has been said concerning apparatus might be repeated concerning methods.

It is remarkable that the vast majority of methods and apparatus are contrived to furnish a visible result. Sight has long been acknowledged by science as the supreme sense. Perhaps the philosopher was right who asserted that nothing is really known until it is presented in a visible form. We biologists can not deplore too frequently or too emphatically the great mathematical delusion by which men often of very great, if limited, ability have been misled into becoming advocates of an erroneous conception of accuracy. Although I have expressed myself on the subject before its importance justifies recurring to it. The delusion is that no science is accurate until its results can be expressed mathematically. The error comes from the assumption that mathematics can express complex relations. Unfortunately, mathematics have a very limited scope and are

based upon a few extremely rudimentary experiences, which we make as very little children and of which probably no adult has any recollection. The fact that from this basis men of genius have evolved wonderful methods of dealing with numerical relations should not blind us to another fact, namely, that the observational basis of mathematics is, psychologically speaking, very minute compared with the observational basis of even a single minor branch of biology. Moreover, mathematics can at the utmost deal with only a very few factors and can not give any comprehensive expression of the complex relations with which the biologist has to deal. While, therefore, here and there the mathematical methods may aid us, we need a kind and degree of accuracy of which mathematics is absolutely incapable. For our accuracy it is necessary often to have a number of data in their correct mutual relations presented to our consciousness at the same time, and this we accomplish by the visual image, which is far more efficient for this service than any other means of which we dispose. When we wish to understand a group of complex related details, such as an anatomical structure, we must see them, and if we can not see them no accurate conception of the group can be formed. With human minds constituted as they actually are, we can not anticipate that there will ever be a mathematical expression for any organ or even a single cell, although formulæ will continue to be useful for dealing now and then with isolated details. Moreover, biologists have to do with variable relations, some of which of course can be put into mathematical form, but we find that even the simplest variations become clearer to us when presented graphically. The value to every student of science of the graphic method has been immense. Biologists can work to advantage with quantitative methods, we welcome the increasing use of measurements in biology, we welcome the English journal *Biometrika*, the organ of the measuring biologists—but none the less we refuse to accept the mathematical delusion that the goal of biology is to express its results in grams, meters and seconds. Measurements furnish us with so-called "exact" records, but the aim of science goes beyond the accumulation of exact records to the attainment of accurate knowledge, and the accuracy of our knowledge depends chiefly on what we see. The practise of science conforms to this principle, the definite affirmation of which may prove of continuing advantage.

No class of records illustrates the value of sight in science more impressively than those made by instruments for registering the time factor. The kymographion invented by Carl Ludwig is the prototype of many apparatus. In them all a succession of events, like heart beats for example, together with marks showing the time are so registered that they can be seen simultaneously and thus readily compared. If no such apparatus were available much of our most important scientific knowledge would not exist. To deprive mankind of microscopes or telescopes would be hardly a more serious blow to science. We do not of course depend on our eyes for the notion of time—for the congenitally blind perceive time—but as soon as we wish to know accurately the relation of changing events to time intervals we depend upon having them recorded in a visible form. It is the practical acknowledgment of the superiority of the eye as an agent to make clear the correlation of data.

When we refer to the history of modern medical science we begin with the anatomist Vesalius, because he reintroduced reli-

ance on seeing in place of reliance on the reading of old authorities.

To dilate longer before this section of the American Association upon the value of seeing is superfluous. We have all been trained by dissection and by looking through the microscope, and we will not deny our training, which many of us are engaged in perpetuating.

Scientific records have a far wider scope than ordinary business records, which merely put down details that can not be carried in the memory. Science strives constantly after new ways of recording and demonstrating facts, which would otherwise be imperfectly known, or not known at all, and at the same time of eliminating the personal factor, by getting the data into a form to assist others in the work of verification.

Scientific men base their work upon a series of assumptions: first, that there is absolute truth, which includes everything we know or shall know; second, that we ourselves are included in this absolute truth; third, that objective existence is real; fourth, that our sensory perception of the objective is different from the reality. These conceptions constitute our fundamental maxims, and even when not definitely put in words they guide all sound scientific research. Metaphysicians find such maxims interestingly debatable, but science applies them unhesitatingly and is satisfied because their application succeeds. Philosophy, ever a laggard and a follower after her swifter sister, has lately and somewhat suddenly termed the scientific habit of work pragmatism and has taken up the discussion of it with delightful liveliness. Let us acknowledge the belated compliment and continue on our way.

The practical result of the four maxims has been that we further assume that all errors are of individual human origin and that there are no objective errors. We make *all* the mistakes, nature makes none. To render the pursuit of new knowledge successful our basic task is to eliminate error, or in other words to decide when we have sufficient proof. The elimination of error depends primarily upon insight into the sources of error, which, since methods of all sorts are employed, involves an intimate technical acquaintance with the methods, with just what they can show, with what they can not show and with the misleading results they may produce. In the laboratory training of a young scientific man, one chief endeavor must always be to familiarize him with the good and the bad of the special methods of his branch of science. Not until he thoroughly understands the character and extent of both the probable and the possible errors is he qualified to begin independent work. His understanding must comprise the three sources of observational error, namely, the variation of the phenomena, the imperfections of the methods and the inaccuracy of the observer. The personal equation always exists, although it can be quantitatively stated only in a small minority of cases.

The history of science at large, the history of each branch of science and the personal experience of every active investigator all equally demonstrate that the greatest source of error is in our interpretations of the observations, and this difficulty depends, it seems to me, more than upon any other one factor, upon our unconquerable tendency to let our conclusions exceed the supporting power of the evidence. Since generalization is the ultimate goal, we are too easily inveigled into assuming probabilities to be certainties, and into treating theories and even hypotheses as definite conclusions. Each generation of investigators in its turn spends

much time killing off and burying older erroneous interpretations. The business is seldom accomplished by direct attack, for error perishes only in the light of truth, as microorganisms are said to perish suddenly when struck by ultra-violet rays. Owing to the load of false theories, we work like a mental chain-gang and are never unfettered. The handicap imposed by wrong hypotheses has always impeded the growth of science. Allusion to a few celebrated instances will suffice. Phlogiston long prevented chemistry from becoming the peer of other sciences. It was a notion which remained alive and dominant until Lavoisier rendered it a mere historical curiosity, by discovering the true principle of combustion. The corpuscular theory of light, upheld by Newton, long retarded physics. It was got rid of, not by proving it false, but by proving the undulatory theory true. The doctrine of the special creation and fixity of species was universally accepted, although utterly without justification. It vanished from science when the true doctrine of evolution was convincingly established. The hypothesis that great epidemics are due to diseases spread by smell, although only the bad guess of ignorance, lasted until modern bacteriology showed us the real causes of infection.

The multitude of such experiences, great and small, has gradually created among scientific men a special highly characteristic mental attitude. They regard the majority of the accumulated data and many of the inductions of science as correct. This is their estimate of the great body of information which, though personal in its origin, has been in the course of time, so tested and verified that it is looked upon as established and secure. When Asellus in 1622 discovered the lymphatics or so-called lacteals of the mesentery and demonstrated that they convey products of digestion from the intestine, his knowledge was his own, and at first his only. Since then the observations have been so repeatedly verified and of course extended that all uncertainty has vanished from our minds. Similarly in innumerable other cases reasonable impersonal certainty has been attained. Yet the investigator lives in an atmosphere of concentrated uncertainty, for he is convinced that at any time new data may turn up, and that all generalizations are likely to require modification. We might well adopt as our cry—Incredulity towards the known; open credulity towards the unknown.

We think of science as a vast series of approximations and our task is constantly to render our approximations closer to absolute truth, the existence of which we take for granted. We use our approximations as best we may, treating them in large part and at least for the time being as if they were accurately true, yet meanwhile we remain alert to better them. This has long been the standard of scientific thought. It is the pragmatic attitude of mind, but its new name has not rendered it a novelty.

The pivot of all research is adequate proof. It would certainly aid science if some competent philosopher should make a study of the practise of investigators in the various branches of science sufficient to render clear the general principles, by which investigators decide when a new observation or a new induction is sufficiently proven. If we follow the advance of research in any particular direction we soon realize that there is a more or less definite standard of proof, which, though never clearly formulated, is none the less insisted upon, so that any paper which does not come up to this standard is subject to unfavorable criticism. Two elements of this

standard we know, the first the elimination of the recognized sources of error, second the repetition of the observations so that the constancy of the phenomenon is assured. We can not do more than allude to this theme, which I must leave to the future and to a more competent mind to analyze and develop.

To sum up: The method of science is not special or peculiar to it, but only a perfected application of our human resources of observation and reflection—to use the words of von Baer, the greatest embryologist. To secure reliability the method of science is *first,* to record everything with which it deals, the phenomena themselves and the inferences of the individual investigators, and to record both truly; *second,* to verify and correlate the personal knowledges until they acquire impersonal validity, which means in other words that the conclusions approximate so closely to the absolute truth that we can be safely and profitably guided by them. The method of science is no mystic process. On the contrary, it is as easily comprehended as it is infinitely difficult to use perfectly and at its best the method supplies merely available approximations to the absolute.

We set science upon the throne of imagination, but we have crowned her with modesty, for she is at once the reality of human power and the personification of human fallibility.

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THE FORMATION OF CARBOHYDRATES IN THE VEGETABLE KINGDOM[1]

THE classical discovery of Woehler in 1828 first revealed to chemists the possibility of the synthetic production of those compounds which occur naturally in the members of the animal and vegetable kingdoms. Woehler himself evidently realized the importance of his discovery. Thus, in a letter to his old teacher, Berzelius, he wrote:[2]

> You may remember how, while I was with you, when trying to make ammonia combine with cyanic acid, I always obtained a crystalline body which gave the reactions of neither the one body nor the other. I have just made this crystalline body the subject of a little investigation, preparing it by the action of ammonia on lead cyanate and have discovered it to be nothing less than urea.

Then he significantly adds, "This may be taken as an artificial production from inorganic substance."

The idea, however, that such compounds could be formed only through the agency of the vital forces of the living organism was one of such long standing and was so deeply established in the popular belief that even the chemists contemporaneous with Woehler were slow to grasp the full significance of the discovery. Berzelius himself was evidently not convinced, since in his text-book published in 1837, nine years after Woehler's discovery, he expressed doubt as to the possibility of being able to discover the differences between the causes of reactions in the living organism and those in the inorganic realm. Likewise Gerhardt[3] wrote seven years later (1842) as follows: "I have shown that the chemist works in a way altogether opposite from living nature. The one burns, destroys, operates by analysis. Vital force alone operates by synthesis and reconstructs the edifice torn down by chemical forces."

Other discoveries, however, of a nature

[1] Address of the vice-president and chairman of Section C—Chemistry—at the Minneapolis meeting of the American Association for the Advancement of Science.

[2] "Berzelius-Woehler Briefwechsel," I., p. 206; Armitage, "A History of Chemistry," p. 143.

[3] *Compt. rend.*, 15, p. 498. Bunge, "Text-book of Organic Chemistry," p. 1.

similar to Woehler's followed, although somewhat tardily. Kolbe in 1845 showed that it was possible to synthesize acetic acid from its elements. Berthelot in 1854 likewise built up the fats. In the light of such achievements the most skeptical could no longer maintain the old view concerning the impossibility of building up in the laboratory from inorganic sources those compounds naturally occurring in the living organism. The recognition of this great truth opened up to chemists a new line of investigation and from that time the synthetic preparation of organic compounds has ever been a fruitful field of research. It has sometimes seemed to me, however, that in the enthusiasm over the discoveries which chemists have made in this field of investigation we are inclined to over-estimate the work done, great as that may be, and perhaps unintentionally convey the impression that the chemical changes taking place in the living organism are thoroughly understood and can be duplicated in the laboratory. While of course it is true that many of the compounds in the living organism have been synthesized and that the number of such syntheses is constantly increasing, yet we must not forget that the chemist's method has never been, in detail at least, the method of nature. Indeed, as a rule they are widely divergent. We are apt to employ powerful reagents many of which so far as is known do not exist in the organism nor in the mediums from which it draws its sustenance. The drastic treatment to which these substances are often subjected and the temperatures at which the reactions are carried on are all in the greatest contrast to the conditions which prevail in the organism in which the natural synthesis is effected. In a few cases the laboratory methods employed more nearly approach the conditions which prevail in nature and such syntheses always have a greatly added interest at least from a theoretical standpoint. Thus the observation of Loew[4] that a sugar-like compound could be formed by the condensation of formaldehyde was exceedingly important, but this importance was greatly enhanced by the fact that this condensation could be effected by the action of mild alkalis or even neutral substances at ordinary temperatures—conditions which approximate those existing in the growing plant. Likewise the importance of the observations of Lobry de Bruyn and van Ekenstein[5] that the three hexoses, namely, mannose, fructose and dextrose, are mutually convertible, lay largely in the fact that these transformations could be made to take place at ordinary temperatures under the influence of reagents that may exist in the soil. The study of such problems as these has become not only one of the most alluring and fascinating in the fields of research in organic chemistry, but their solution is fraught with the greatest significance, since it will be a step in the direction of gaining some understanding of the mysteries of life itself.

Inasmuch as the carbohydrates play such an important part in the economy of the vegetable kingdom, it is quite natural that the investigations of the problems pertaining to the synthesis of compounds in the living organisms have been largely directed towards this class of compounds. Since it is impossible in a short time to discuss with any thoroughness the various researches carried out in this field of investigation I will confine myself to those which have some important bearing upon the well-known hypothesis proposed by Baeyer, with a view of determining, if pos-

[4] *J. prakt. Chem.*, 33, p. 321.
[5] *Ber. d. chem. Gesell.*, 28, p. 3078.

sible, its standing in the light of modern discoveries.

In 1861 there appeared in the *Annalen der Chemie* and simultaneously in the *Comptes rendus* a short article by a Russian chemist, Butlerow,[6] in which he described the formation of a sugar-like substance (methylenitan, he termed it) through the interaction of trioxymethylene and calcium hydroxide in an aqueous solution. Although the resulting substance was optically inactive and was not fermentable, Butlerow nevertheless was sure that the sweet syrup which he obtained contained a sugar, for he concludes his accounts with the statement that his results furnish the first example of the synthesis of a sugar-like body. These results of Butlerow proved to be of the greatest importance, not only because of the facts revealed, but also because they served to suggest to other investigators the method of attacking the general problem of the synthetic production of sugars. Moreover, they undoubtedly served as an experimental groundwork upon which Baeyer a few years later based his well-known hypothesis.

The essential assumptions of Baeyer's hypothesis, viz., the production of formaldehyde through the interaction of carbon dioxide and water, and the subsequent polymerization of this to a sugar, are well known to all and require no discussion. It may be of interest, however, to recall some of the reasons advanced by Baeyer in setting forth these assumptions.

The original statement of the theory was published in the *Berichte* just forty years ago (1870) and was entitled "Ueber die Wasserentziehung und ihre Bedeutung für das Pflanzenleben un die Gährung."[7]

[6] *Annalen der Chem.*, 120, p. 295; *Compt. rend.*, 53, p. 145.

[7] *Ber. d. chem. Gesell.*, 3, p. 67,

Baeyer here calls attention to the fact that in the transformation of the compounds of carbon, hydrogen and oxygen, the separation of water plays just as weighty a rôle as do the processes of oxidation and reduction. He then discusses the groups of those reactions in which water is formed by the union of hydrogen and oxygen withdrawn from the interacting compounds. Among the various reactions are those in which the water removed is formed by the union of hydrogen taken from one molecule with an hydroxyl group taken from a different molecule—an "*äussere Condensation*," as Baeyer termed it. To illustrate this he gives his interpretation of the reaction involved in Butlerow's sugar synthesis. Although Butlerow had used trioxymethylene, Baeyer evidently felt justified, because of the intimate relation between this compound and formaldehyde, in assuming that the reaction actually taking place is one between the different molecules of formaldehyde. Considering that formaldehyde in aqueous solution is in the hydrated form, Baeyer shows that by the splitting off of water formed by the union of a hydroxyl group from one molecule of formaldehyde with the hydrogen from another molecule and the union of the two remaining residues through the resulting free affinities, it is easy to understand how from six molecules of formaldehyde the hexoses might result. Baeyer even deduced two possible structural formulas for a hexose based on this interpretation of the course of the reaction, one of these being the well-known aldehyde formula. Likewise he shows how by the condensation of three molecules of formaldehyde one would expect to obtain an aldehyde of glyceric acid. The older view of Liebig to the effect that the fruit acids are the intermediate products from which the sugars result, Baeyer rejects in favor of

the one that the sugars are formed directly from the carbonic acid. This formation could be satisfactorily accounted for as follows: Through the combined action of chlorophyll and sunlight carbon dioxide suffers dissociation into carbon monoxide and oxygen. The oxygen is evolved while the carbon monoxide combines with the chlorophyll much as it does with the hæmoglobin of the blood. By the action of hydrogen, obtained by the dissociation of water, the carbon monoxide is then changed into formaldehyde from which the sugar results by condensation. The essential points in Baeyer's hypothesis are therefore, first, the production of formaldehyde from carbon dioxide and water, and second, the formation of sugars by the polymerization of the aldehyde. Other investigators have suggested certain modifications of this theory, mainly in the method of generation of the formaldehyde. Thus, Erlenmeyer[8] in 1877 from certain observations made on the action of water on hydroxy acids was led to conclude that the carbonic acid would be acted upon by water under the conditions existing in the plant with the production of formic acid and hydrogen peroxide in accordance with the following equation: $HO.COOH+HOH \rightarrow H_2O_2 + H.COOH$. The formaldehyde would then result from the reduction of the formic acid. Bach,[9] on the other hand, assumes the decomposition of the carbonic acid directly into formaldehyde and an unstable percarbonic acid, H_2CO_4, which would break down into hydrogen peroxide with regeneration of carbon dioxide. Usher and Priestley[10] as well as Pollacci[11] have also suggested certain modifications of the general assumptions.

[8] *Ber. d. chem. Gesell.*, 10, p. 634.
[9] *Compt. rend.*, 116, pp. 1145, 1389; 126, p. 479.
[10] *Proc. Royal Soc.*, B, 77, p. 369; 78, p. 318.
[11] *Bot. Zentralbl.* for 1904 and 1905.

The principal researches which have a direct bearing on the validity of Baeyer's hypothesis may be classified in regard to their bearing on the following problems: First, Is it possible to produce formaldehyde in the laboratory from carbon dioxide and water under conditions approximating those existing in the plant? Second, Is formaldehyde actually present in the living organism? Third, Is it possible for the organism to assimilate formaldehyde? Fourth, Is it possible to produce carbohydrates directly from formaldehyde?

It is my purpose to discuss briefly the more important researches carried out in these fields of investigation.

PRODUCTION OF FORMALDEHYDE THROUGH THE REDUCTION OF CARBONIC ACID

The production of formaldehyde through the reduction of water and carbon dioxide has been the subject of numerous researches. Maly[12] in 1865, and in more recent time Lieben[13] (1895), attempted this reduction by employing various amalgams as reducing agents. Royer[14] in 1870 and Coehn and Jahn[15] in 1904, on the other hand, studied the effect of electrolytic hydrogen on the carbonic acid. In all these cases, however, the reduction resulted not in the formation of formaldehyde, but of formic acid. In 1893 Bach[16] attempted to carry out the reaction under conditions closely approximating those existing in the plant. As a substitute for the chlorophyll in the plant Bach, relying upon the well-known sensitiveness of uranium compounds to light, used uranium acetate as a chemical and photo-sensitizer. Carbon dioxide was passed through flasks filled

[12] *Annalen der Chem.*, 135, p. 119.
[13] *Wien. Monats.*, 16, p. 211; 18, p. 582.
[14] *Compt. rend.*, 70, p. 731.
[15] *Ber. d. chem. Gesell.*, 37, p. 2836.
[16] *Compt. rend.*, 116, pp. 1145, 1389.

with a 1.5 per cent. solution of uranium acetate. When the experiment was carried out in the sunlight there was obtained a precipitate of the hydroxides of uranium together with a small amount of peroxide. The results are explained on the supposition that the carbonic acid is resolved into formaldehyde and a percarbonic acid, H_2CO_4, in accordance with the following equation: $3H_2CO_3 = CH_2O + 2H_2CO_4$. The percarbonic acid as fast as formed breaks down into hydrogen peroxide and carbon dioxide. To support this interpretation of the course of the reaction, Bach refers to the work of Wurster,[17] who claimed to have demonstrated the presence of hydrogen peroxide in plants, although Wurster's results had been strongly challenged by Bokorny.[18] In 1898 Bach[19] again returned to the problem, this time using palladium as a catalyzer. Carbon dioxide was passed through water containing palladium held in suspension. In the clear liquid obtained by filtering off the palladium Bach claims to have established the presence of formaldehyde.

While the experiments of Bach are ingeniously conceived and are of great interest, his interpretation of the results do not appeal to one as at all conclusive. The most that can be said is that the generation of formaldehyde and hydrogen peroxide under the conditions of the experiments is probable. Their presence must be regarded as inferred rather than proved. It is not strange therefore that his results have been criticized. In 1904 Euler[20] repeated the work and concluded that the carbon dioxide in Bach's experiments did not enter into the reaction at all since exactly the same results are obtained if one substitutes a current of hydrogen or nitrogen for the carbon dioxide.

On the other hand, Usher and Priestley[21] in 1906 reported that "the experiments of Bach have been repeated and confirmed, both as to the production of peroxide and formaldehyde." Oddly enough, however, they gave no account as to their manner of confirming these results, and since Bach himself rather inferred than proved the presence of these two compounds the statement of Usher and Priestley is not wholly satisfactory. In order to meet the criticism that the production of formaldehyde in Bach's experiments may have resulted from the reduction of the acetic acid which would undoubtedly be formed by the hydrolysis of the uranium acetate used, Usher and Priestley[22] in a later article described a series of experiments in which uranium sulphate was substituted for the uranium acetate. In these experiments no formaldehyde could be detected, although the authors report that "a study of the reactions involved favors the view that it is formed as a transitory intermediate product." In their original work Usher and Priestley were led to conclude from experiments on plants (*Elodea* were used) that the generation of formaldehyde in the plant from carbon dioxide and water is not a vital process at all since small plants of *Elodea* in which all life had been destroyed by immersion in boiling water accumulated perceptible amounts of formaldehyde when exposed to sunlight in a moist atmosphere of carbon dioxide. The tests for formaldehyde were made directly upon the leaves as well as upon the distillate from the leaves. The principal tests employed were (*a*) the development of color in Schiff's reagent, (*b*) the formation of methyleneaniline with

[17] *Ber. d. chem. Gesell.*, 19, p. 3195.
[18] *Ber. d. chem. Gesell.*, 21, p. 1100.
[19] *Compt. rend.*, 126, p. 479.
[20] *Ber. d. chem. Gesell.*, 37, p. 3411.
[21] *Proc. Royal Soc.*, B, 77, p. 370.
[22] *Proc. Royal Soc.*, B, 78, p. 318.

aniline, and (*c*) the formation of hexamethylene tetramine with ammonia and bromine. Acting on the conclusion that the production of formaldehyde in the plant is not a vital process, these investigators in a later article described a series of experiments in which they attempted to produce formaldehyde from water and carbon dioxide in the laboratory by reproducing the conditions existing in the plant. Plates of glass were painted over with gelatine, which in turn was coated with a thin film of chlorophyll. The plates so prepared were placed in a moist atmosphere of carbon dioxide and exposed to the sunlight. The chlorophyll was employed to effect a reduction of the carbon dioxide and water into formaldehyde by acting as a photo-sensitizer, while the gelatine was used in the hope that it would absorb all formaldehyde as fast as formed, thus removing it from the sphere of action. After the exposure of the plate the gelatine was removed and tested for formaldehyde by the tests previously mentioned and in all cases the aldehyde was found to be present.

If the interpretation of these results, as given by Usher and Priestley, is correct, this work is of the very highest value, not only in its relation to Baeyer's hypothesis, but especially because the reduction of carbon dioxide and water to formaldehyde in plants is shown to be a laboratory and not a vital process. It must be stated, however, that the conclusions reached have been contradicted by Ewart,[23] who insists that the gelatine used in the experiments will give the test for formaldehyde just as well before exposure to carbon dioxide and light as after such exposure; and that even granting the generation of formaldehyde under these conditions, it is, to say the least, just as reasonable to conclude that it is derived from the decomposition of the chlorophyll in the presence of oxygen as it is to conclude that it is formed from carbon dioxide and water.

Some of the most interesting results in this field of investigation have been obtained by Löb,[24] who attempted to gain some insight into the changes taking place in the plant by substituting the action of the silent electric discharge for that of the sunlight as well as of any catalytic agent or enzyme present in the plant. In this way it was shown that formaldehyde is a direct reduction product of carbon dioxide and water vapor. Löb claims, and with justice, that his experiments bring the first positive proof that formaldehyde is a direct reaction product of moist carbon dioxide.

In 1907 Fenton[25] attempted the generation of formaldehyde directly from carbonic acid by passing a current of carbon dioxide for a number of hours through pure water in contact with several rods of amalgamated magnesium. He concludes that "the solution gives slight but unmistakable indications of formaldehyde" with certain standard color tests. So far as I know, these results have not been challenged.

The most recent work, as well as probably the most significant, is that of Berthelot and Gaudechon.[26] By the action of ultra-violet light on a mixture of carbon dioxide and water vapor, these investigators have succeeded in obtaining formaldehyde together with carbon monoxide and oxygen. Under the same conditions, carbon monoxide and water vapor gives rise to formaldehyde, carbon dioxide and hydrogen.

[23] *Proc. Royal Soc.*, B, 80, p. 30.

[24] *Zeit. f. Elektrochem.*, 12, p. 282.

[25] *J. L. Chem. Soc.*, 91, p. 687.

[26] *Compt. rend.*, 150, p. 1690.

THE EXISTENCE OF FORMALDEHYDE IN PLANTS

As an immediate result of Baeyer's hypothesis attempts have been made to detect formaldehyde in plants of various species and grown under various conditions. The results of these investigations are not at all satisfactory and indeed consist largely in affirmations and denials. The problem, however, is not a simple one. In the first place formaldehyde, if present at all, can only be present in very minute quantities because of its very great toxicity, and while it is true that extremely delicate tests have been advanced for formaldehyde it is likewise true that these tests have been subjected to considerable criticism. Moreover, it is possible that such delicate tests might be influenced by the presence of various other substances present in the plant.

In 1881 Loew and Bokorny[27] advanced the theory that the vital force of the living protoplasm is essentially connected with the presence of aldehyde groups in the substance forming the protoplasm. When certain algæ, for example, were immersed in a dilute solution of silver nitrate the living cells were found to be darkened when examined under the microscope. The authors attribute this change in color to the reduction of the silver salt by the aldehyde present. Plants previously immersed in boiling water to destroy the life of the protoplasm failed to give the test; hence the authors conclude that the aldehyde group disappears with the life of the plant.

In the same year Reinke[28] by macerating certain green leaves (those of the grape vine, the poplar, the willow and conifers, were tested) and distilling the mass in a current of steam obtained a liquid having strong reducing powers which he attributed to the presence of an aldehyde. Moreover, the aldehyde must be an easily volatile one, for it is present chiefly in the first portions of the distillate. While not able to prove the exact identity of the aldehyde Reinke concluded that it is formaldehyde. These reactions were obtained only in the case of chlorophyll-bearing plants. Reinke suggested that the reducing power of the algæ observed by Loew and Bokorny might be due to the presence of formaldehyde in the plant. This statement led to further studies by both investigators and the publication of several articles[29] in which each held to his original interpretation of the results, although Reinke was forced to admit that he had not definitely proved the presence of formaldehyde in the distillate from the plants, but only the presence of an easily volatile aldehyde.

Mori[30] (1882) experimenting with the leaves of the rose bush and of oats obtained results similar to those of Reinke. He also tested the plant by adding a few drops of Schiff's reagent directly to portions of the plant, whereupon a red color gradually resulted. Loew and Bokorny[31] claim, however, that the development of the color in Mori's experiments was due to the evaporation of the sulphurous acid in Schiff's reagent and show that this reagent gradually becomes colored by mere exposure to air.

In 1889 Polacci[32] reported the results of an extended series of investigations from

[27] *Arch. ges. Physiol.* (*Pflügers*), 25, p. 150.

[28] *Ber. d. chem. Gesell.*, 14, p. 2144.

[29] *Bot. Zeit.*, 1882, No. 40; *Ber. d. bot. Gesell.*, 15, p. 201; 17, p. 7; *Ber. d. chem. Gesell.*, 15, pp. 107, 695; "Studien über das Protoplasma," zweite Folge, Berlin, 1883.

[30] *Nuovo. Gio. Bot. Ital.*, 14, p. 147.

[31] *Bot. Zeit.*, 1882, p. 832; *Arch. ges. Physiol.* (*Pflügers*), 26, p. 50.

[32] *Atti d. Inst. Bot. d. Univ. d. Pavia*, II., Serie 7, 1.

which he concluded that formaldehyde is undoubtedly present in the green parts of living plants when exposed to sunlight. He experimented directly with the growing plant, as well as with the liquid obtained by distilling the leaves in a current of steam. Twigs were bent down from the growing plant into cylinders containing Schiff's reagent. It was found that when the experiment was carried on in the sunlight the red color gradually developed, which result was attributed by the investigator to the production of formaldehyde. While the results obtained by the tests made directly upon the leaves were only suggestive and not conclusive, the distillate from the leaves Polacci affirms undoubtedly contains formaldehyde itself. Many tests were employed. Some of these were of a general character, while others were definite for formaldehyde itself. Special stress is placed on (*a*) the color test obtained by successive additions of phenylhydrazine, nitroprussiate of sodium and alkali; (*b*) the color test with sulphuric acid and codeine; (*c*) Trillat's color test with an aqueous solution of dimethyl aniline in the presence of acetic acid and lead peroxide; (*d*) the reaction with phenylhydrazine; (*e*) the color test with phenol and sulphuric acid. A short time after the publication of the results of Polacci's experiments two other Italian investigators, namely Plancher and Ravenna,[33] reported a series of similar experiments, but failed to find any conclusive proof of the presence of formaldehyde in plants. The formation of color in Schiff's solution under the conditions of Polacci's experiment was attributed not to the presence of formaldehyde, but to the "active" oxygen evolved by the plant in the process of assimilation. They likewise claimed that the tests which Polacci employed for the detection of formaldehyde in the distillate from green leaves are untrustworthy. In order to determine whether formaldehyde, if present in the green leaves, would actually distill over or would remain combined with compounds in the plants, these investigators added a very dilute solution of formaldehyde to leaves before distillation and then tested for formaldehyde in the distillate. The results showed that the amount of formaldehyde which must be present in the leaves in order to respond to conclusive tests in the distillate is so large that it would destroy at once the vitality of the plants. Polacci's results have also been criticized by Czapeck[34] as being indefinite. Euler,[35] on the other hand, confirms his results, although he expresses the opinion that the formaldehyde is not present in a free state in the plant, but is liberated from its condensation products by the process of distillation. In a more recent article Polacci[36] refers to the criticisms of his investigations and defends his original conclusions with great vigor.

Among the other investigators may be mentioned Grafe[37] as well as Kimpflin,[38] both of whom maintain that formaldehyde is undoubtedly present in the growing plant. The former investigator used a solution of diphenylamine in sulphuric acid as a test, claiming that this reagent gives a green coloration with exceedingly minute quantities of the aldehyde. Kimpflin, on the other hand, recognizing that formaldehyde, if present at all, can only be present in minimal amounts, ingeniously attempts to store up the compound as fast as formed until a sufficient quantity is obtained to respond to the tests. To do

[33] *Atti d. Real. Accad. d. Lincei*, 13, p. 459.

[34] *Bot. Zeit.*, 1900, p. 153.

[35] *Ber. d. chem. Gesell.*, 37, p. 3411.

[36] *Atti d. Real. Accad. d. Lincei*, 16, p. 199.

[37] *Oesterreich. bot. Zeitschrift*, 1906, No. 3.

[38] *Compt. rend.*, 144, p. 148.

this he introduces into the tissue by means of a capillary tube a solution of sodium acid sulphite and methylparamidometacresol. After this injection the plant is exposed to the light for a time, when a section of the leaf is placed in absolute alcohol and subsequently examined under the microscope in the presence of a drop of water. The presence of the aldehyde is shown by the formation of a red color. According to the author, the aldehyde as fast as produced combines with the acid sulphite to form the addition compound which is stable in the absolute alcohol. Upon the addition of a drop of water, however, the aldehyde is liberated and forms a characteristic color with the cresol.

While Usher and Priestley[39] claim to have definitely proved the presence of formaldehyde in the dead plant according to the method described above, they merely infer its presence in the living plant. In fact, they assert that it would be useless to look for it in a healthy living plant because of the rapidity with which it would be transformed into other substances.

In a recent article Bokorny[40] criticizes the work of Kimpflin and others and maintains that the amount of formaldehyde which would have to be present in the plant in order to respond to any of the known tests would certainly destroy the vitality of the plant. He maintains, however, that it is undoubtedly present, but in such minute quantities at any given period that the only hope for its detection lies in the discovery of some agent which would combine with the aldehyde as fast as generated to form a compound that is not toxic to the plant and from which compound it could again be recovered in quantities sufficient to respond to the standard tests. It may be added that this is practically what Kimpflin attempted.

[39] *Proc. Royal Soc.*, B, 77, p. 370.

[40] *Arch. ges. Physiol.* (*Pflügers*), 125, p. 484.

THE ASSIMILATION OF FORMALDEHYDE BY PLANTS

No single evidence perhaps would count so much towards corroborating Baeyer's hypothesis as the proof of the power of plants to assimilate free formaldehyde with the production of carbohydrates. It is natural therefore that many experiments have been made to ascertain just what effect formaldehyde has on the growing plant. In conducting such experiments naturally the formaldehyde would have to be administered in very dilute solutions; moreover, the amount of such solutions would have to be large in order to obtain a sufficient amount of formaldehyde to effect a sensible amount of any assimilation product.

The first investigator to make any extensive study of this question was Bokorny,[41] who attempted to grow certain water plants (green filaments of *Spirogyra*) in dilute solutions of formaldehyde. He found that formaldehyde, even in solutions of 1 to 50,000, was fatal to the growth of the plant. Next the attempt was made to substitute for the free formaldehyde some substance which under the influence of the plant would slowly decompose, giving formaldehyde as one of the decomposition products. Such a substance Bokorny found in methylal which decomposes into formaldehyde and methyl alcohol and also in the sodium acid sulphite addition product of formaldehyde. By the use of these compounds Bokorny hoped to diminish the concentration of the actual aldehyde to the minimum and yet by its constant formation furnish a sufficient amount of it to

[41] *Ber. d. bot. Gesell.*, 1888, p. 119; *Chem. Zeit.*, 44, p. 525; *Phar. Post*, 36, p. 153; *Biolog. Centralbl.*, 12, No. 16 and 17.

the plant to enable one to prove whether or not assimilation actually takes place. It was found that the *Spirogyra* immersed in a solution of either of these substances under certain conditions continue to grow and produce starch in the absence of carbon dioxide. These results must be regarded as strong corroborative evidence in favor of the view that plants have the power of directly assimilating formaldehyde; they are not conclusive, however, since there is no actual proof that the compounds are decomposed by the plants previous to assimilation.

Later Bouillac and Giustiniana[42] succeeded in growing ordinary white mustard in solutions containing traces of formaldehyde. It is interesting to note that a certain amount of light was necessary, however, for the growth of the plant.

Treboux[43] also reports that he has successfully grown the *Elodea* in solutions of one part of formaldehyde in one hundred thousand, but that no starch was formed; hence he concludes that his results are opposed to the general belief that formaldehyde is directly polymerized to carbohydrates.

Usher and Priestley[44] in the investigations referred to above likewise report a case of starch formation in a solution containing one part of formaldehyde in one hundred thousand.

In 1908 Bokorny[45] reported some experiments in which he succeeded in proving that spirogyra can assimilate such substances as glycerol, sucrose and even traces of formaldehyde itself when present in dilute solutions. A year later[46] he again reported some experiments in which he attempted to grow water cress under a bell jar over a 30 per cent. solution of sodium hydroxide containing small amounts of formaldehyde and concludes that the upper portions of the plant undoubtedly absorb aldehyde vapor. This assimilation can take place in the absence of both oxygen and light.

Likewise Grafe and Vieser[47] have grown seedlings of *Phaseolus vulgaris* in air free from carbon dioxide but containing formaldehyde and report that plants under such conditions grow more rapidly than in normal air.

So far as I know, no attempts have been made to ascertain the effect of glycolaldehyde and glycerose upon the growth of the plant. It is probable that these compounds are intermediate products in the formation of a sugar by the polymerization of formaldehyde. One would naturally expect therefore that they would be assimilated by the growing plant. Moreover, they would be better adapted than formaldehyde for such investigations, since they are relatively less toxic, and hence could be used in larger amounts.

SYNTHETIC PRODUCTION OF SUGAR FROM FORMALDEHYDE

It will be recalled that Butlerow's methylenitan was synthesized not from formaldehyde itself but from a closely related compound, trioxymethylene. Loew[48] in 1886 was the first to build up a sugar directly from formaldehyde, using mild alkalis as condensing agents. Three years later Loew[49] succeeded in obtaining a purer product (formose) by using the oxides of lead and magnesium as condensing agents. In the meanwhile Fischer[50] was carrying out those brilliant researches in which the

[42] *Compt. rend.*, 136, p. 1155.
[43] "Flora," 92, p. 73.
[44] *Proc. Royal Soc.*, B, 77, p. 370.
[45] *Arch. ges. Physiol.* (*Pflügers*), 125, p. 467.
[46] *Arch. ges. Physiol.* (*Pflügers*), 128, p. 565.
[47] *Ber. d. bot. Gesell.*, 27, p. 431.
[48] *J. prakt. Chem.*, 33, p. 321.
[49] *Ber. d. chem. Gesell.*, 22, p. 475.
[50] *Ber. d. chem. Gesell.*, 20, pp. 1093, 2566, 3384.

synthesis of fructose was effected first from acrolein and later from glycerol. The whole subject of the action of alkalis upon formaldehyde has been taken up in recent years by Euler,[51] who has made a careful study of the course of the reaction and the conditions affecting it. It is especially interesting to note that this investigator has succeeded in synthesizing a sugar from formaldehyde through the agency of calcium carbonate as a condensing agent. The interest here lies chiefly in the fact that the formation of sugar may be effected by a substance universally distributed in the soil. It is interesting to note that Euler in this way showed that the simplest of sugars, viz., glycolaldehyde, is produced as an intermediate product. The principal sugar finally formed is a pentose, namely, (*dl*)-arabino-ketose. Löb[52] has recently effected similar condensations by the use of zinc as well as zinc carbonate.

Equally important are the results which have been obtained through the action of the silent electric discharge. Berthelot,[53] in this way, produced from a mixture of carbon dioxide, water and hydrogen a substance having the properties of a carbohydrate. With carbon monoxide and hydrogen he obtained a similar product which appeared to be a polymer of formaldehyde. Likewise Slosse,[54] from carbon monoxide and hydrogen, obtained a crystalline, fermentable sugar.

The investigations of Löb[55] in which he produced formaldehyde directly from carbon dioxide and water vapor through the influence of the silent electric discharge have already been referred to. Under the same conditions this investigator has been able to polymerize the aldehyde so formed into glycolaldehyde and this in turn into a hexose. The formation of a sugar from carbon dioxide and moisture has thus been effected through the agency of the energy of the silent electric discharge. It may be added that patents have been taken out for the synthetic production of sugar under this general method.

Taken as a whole, the results of the investigations would seem to corroborate Baeyer's original assumptions. The transformation of carbon dioxide and water into formaldehyde and the subsequent polymerization of this into a sugar under conditions approximating those existing in the plant may be regarded as accomplished. While the evidence advanced can not be regarded as showing beyond doubt the presence of free formaldehyde in the plant, yet it is plain that the failure to detect its presence can not be regarded as fatal or as even opposed to Baeyer's theory. In fact, one would naturally expect that because of its great activity, its polymerization would keep pace with its formation and that the tests for its presence would therefore be negative. On the other hand, it can not be doubted but that plants have the power to directly assimilate formaldehyde. While certain objections have been urged against Baeyer's hypothesis and other radically different ones[56] have been advanced, yet it would seem from the present indications that further progress in our knowledge of the formation of carbohydrates in the vegetable kingdom probably will be made along the lines originally pointed out by Baeyer.

In the study of the results of these investigations one is impressed with the large number of conflicting statements. Certainly one would not turn to these re-

[51] *Ber. d. chem. Gesell.*, 39, pp. 39, 45.
[52] *Biochem. Zeit.*, 12, p. 78.
[53] *Compt. rend.*, 126, p. 610.
[54] *Bull. de l. Ac. roy. de Belg.*, 35, p. 547.
[55] *Zeit. f. Elektrochem.*, 12, p. 282.
[56] Etard, "La Biochem. et les chlorophylles," Paris, 1906; Loew, "Chem. Energ. in leb. Zell.," 1906.

ports as an argument for the exactness of chemical science. As a rule, however, the disagreements relate not so much to the observations as to their interpretation. Of course it is useless to expect investigators to agree upon the question as to whether or not formaldehyde is present in the plant, until they first can agree in regard to the tests for formaldehyde which shall be considered as conclusive. Again while this general subject is primarily a chemical one, yet many of the investigators have been men trained rather in other fields of work. It would seem that the chemist, or better, perhaps, the chemist and the botanist working conjointly, ought to be able to make surer progress in such investigations. The problem is an exceedingly complex one. Its solution involves many reactions at present but little understood—such as the nature of catalytic and enzymic action and the formation of asymmetric compounds. It is probable also that other forces not yet investigated may enter into the reactions by which these compounds are formed. Stewart[57] has even suggested that it is "not improbable that the rotation of the earth or terrestrial magnetism or the motion of the earth around the sun may have some effect." There is no doubt, however, but that progress is being made. It is also undoubtedly true that many of the researches now being carried on in our laboratories will be found to have a more or less direct bearing upon the general question; and it has been partly my object in discussing this topic to emphasize this fact in order that the results of our investigations, whenever applicable, may be directed towards the solution of this problem.

William McPherson
Ohio State University

[57] "Stereochemistry," London, 1907, p. 535.

THE AMERICAN MUSEUM OF NATURAL HISTORY

Dr. Hermon C. Bumpus has resigned the directorship of the American Museum of Natural History and has accepted the position of business manager to the University of Wisconsin. This announcement was made on January 20 by Mr. Seth Low, chairman of a special committee of the trustees appointed to consider the administration of the museum. He gave out the following statement:

> Director Bumpus notified the trustees of the American Museum of Natural History at a special meeting held yesterday afternoon that he had accepted an appointment as business manager for the University of Wisconsin. Accordingly he presented his resignation as director, which was accepted. The administrative difficulty in the museum is thus terminated.
>
> The questions raised as to the respective duty and authority of various officers in the museum seemed to the board important enough to be referred to a special committee, which was appointed on November 30, 1910, to give a hearing to the director and to consider his criticisms. The committee, which consisted of Anson W. Hard, Adrian Iselin, Jr., Percy R. Pyne, Felix M. Warburg and Seth Low (chairman), went into every criticism very thoroughly. They found nothing to justify the sweeping statements which had been made, and the specific criticisms of President Osborn, when sifted, were found to be either unimportant or not sustained. The committee and the board believes that the administration of President Osborn has been wise, efficient, far-sighted and public-spirited, and that the financial management has been sound and constructive.

THE CARNEGIE INSTITUTION OF WASHINGTON

It was announced on January 20 that Mr. Andrew Carnegie had added $10,000,000 to the endowment fund of the Carnegie Institution of Washington. The institution was established in 1902 with a gift of $10,000,000, and Mr. Carnegie recently added $2,000,000. These gifts consist of preferred bonds of the Steel Corporation bearing five per cent. interest and their market value is considerably above their par value. Mr. Carnegie's gifts to

public purposes now amount to about $200,000,000.

SCIENTIFIC NOTES AND NEWS

AT its last meeting the Rumford Committee of the American Academy of Arts and Sciences made the following grants: To Professor Joel Stebbins, of the University of Illinois, $200, in further aid of his researches on the selenium photometer. To Professor M. A. Rosanoff, of Clark University, $300, in further aid of his investigation on the fractional distillation of binary mixtures.

THE Society of American Bacteriologists will meet in Washington, D. C., the last week in December of this year. The officers are: *President,* F. P. Gorham, Brown University; *Secretary,* Charles E. Marshall, Michigan Agricultural College.

AT the annual meeting of the New York Pathological Society, held at the Academy of Medicine, on January 11, the following officers were elected: *President,* Dr. William G. MacCallum; *Vice-president,* Dr. John H. Larkin; *Secretary and Editor,* Dr. A. M. Pappenheimer; *Treasurer,* Dr. Francis C. Wood. Dr. T. M. Prudden and Dr. E. K. Dunham were reelected trustees of the society to serve for a term of three years. The next meeting will be held in conjunction with the Philadelphia Pathological Society in Philadelphia on February 9.

MR. S. P. JONES, formerly assistant state geologist of Georgia, is with the New Jersey Geological Survey.

MR. C. E. BRADLEY has resigned as chemist of the Agricultural Experiment Station at Corvallis, Oregon, to accept the position of research chemist with the Rubber Regenerating Company, of Mishawaka, Indiana.

THE program of the Section of Astronomy, Physics and Chemistry of the New York Academy of Sciences on January 27 included the following papers on Aviation: "Experiences in Aviation," Mr. Clifford B. Harmon; "Practical Utility of Flying Machines," Mr. Hudson Maxim; "The Aeroplane" (illustrated by lantern slides), Lieutenant Phillip Wilcox, U. S. A. R.; "Taking the First Photographs of the Flights of the Wright Brothers at Kitty Hawk, North Carolina" (illustrated by lantern slides), Mr. James H. Hare.

PROFESSOR W. P. MASON, of Rensselaer Polytechnic Institute, delivered a lecture before the Williams College Natural Science Club on "Water and Disease," on January 19.

THE Smithsonian Institution is about to come into possession of a bequest by the recent death of George W. Poore, Esq., of Lowell, Mass. His will provides, after certain minor legacies, that the residue of his estate be given to the Smithsonian Institution to form the Lucy T. and George W. Poore Fund, the income of which is to be used for the purposes for which the institution was founded. The will further requires that this fund shall be kept separate from all other funds and the income to be added to the principal until it shall have reached the sum of $250,000. Mr. Poore explains in his will that he makes this bequest in the hope that "it will form an example for other Americans to follow by supporting and encouraging so wise and beneficent an institution as I believe the Smithsonian Institution to be."

MR. ALCAN HIRSH, a graduate student in electro-chemistry at the University of Wisconsin, has succeeded in producing about half a pound of metallic cerium, one of the rare elements which heretofore has been isolated only in small quantities.

SIR ERNEST SHACKLETON hopes in the course of next year to undertake an expedition to Spitzbergen, spending two and a half or three months in the islands. His party will probably consist of six, including Mr. J. Murray, the biologist, and other members of the *Nimrod* expedition.

HON. CHARLES H. SHERRILL, United States Minister to Argentine Republic, will give an address on February 16 to the officers and students of Columbia University on the opportunities for American engineers in public works and other fields in Argentine.

DR. H. W. WILEY, chief of the division of chemistry of the Department of Agriculture,

lectured before the Chemical Society of Washington and Lee University on January 13, his subject being "Some of the less Obvious Advantages of Chemical Study." Dr. J. W. Mallet, of the University of Virginia, is to lecture before the society on February 17.

Professor George T. Moore recently gave a series of lectures before the Washington University Association on "What goes on in the Ground."

With a greatly increased equipment the new laboratory of St. Luke's Hospital, New York, has recently been opened. Some changes have been made also in the personnel of the laboratory staff, which now includes Dr. Francis C. Wood, director; Dr. Karl M. Vogel, clinical pathologist; Dr. J. Gardner Hopkins, bacteriologist; Dr. William H. Woglom, pathologist; Dr. George C. Freeborn, assistant in pathology, and Dr. N. B. Foster and Dr. H. O. Mosenthal, assistants in chemistry.

The New York Zoological Park has received from Mr. E. B. Bronson, from Quito, Ecuador, a fine specimen of the Spectacled Bear (*Ursus ornatus*), captured in the Andes of Ecuador. Excepting the Æluropus, of eastern Tibet, this is the rarest bear species either alive in zoological gardens or in museums. The specimen is temporarily exhibited in one of the large cages of the small mammal house, where it will remain until the new series of bear dens now under contract and in the course of erection is completed, which will be about June first.

Twenty-three cases of zoological material representing several hundred skins of birds and mammals have been received by the American Museum of Natural History as the first shipment of specimens from the Stefánsson-Anderson Arctic Expedition.

At the last meeting of the Oregon Academy of Sciences a number of new members were elected. The subject of the evening was "The Single Tax," by C. H. Chapman, one of the editors of the *Oregonian*. The meeting was held early in the evening preceded by a dinner which was attended by about sixty-five persons. It was decided to use this plan for a part of the monthly meetings this year combining social and scientific features.

A German edition of "Light Waves and their Uses," by Professor Albert A. Michelson, head of the department of physics of the University of Chicago, has just been issued by the publishing house of Johann Ambrosius Barth in Leipzig. The translation was made by Dr. Max Ikle. The lectures which constitute the book were originally delivered by Professor Michelson in the Lowell Institute in 1899. The translator has added a bibliography of writings bearing on this subject published since 1880.

We learn from *Nature* that the Agenda Club, which was formally inaugurated by a banquet recently, proposes to organize effort, knowledge and influence for the purpose of getting things done which need doing for the benefit of the community. The movement first acquired publicity through "An Open Letter to English Gentlemen" in the *Hibbert Journal*. This letter, and the club itself, appeal frankly to the idealism and the goodwill of the best men; but an equally essential characteristic of the club is to organize the altruism of its members with at least as much efficiency as that of the most successful modern business. The club expressly enunciates its need of guidance by scientific men in determining the agenda to be undertaken and in many details of its work. It is a coordinating society, and not one that overlaps the work of other bodies devoted to special purposes. Among other methods to be employed is that of the most extensive publicity. It contemplates the encouragement of research, especially in social science, and its scheme includes groups of associates, among which are mentioned engineering, literature, medicine and science.

The Berlin correspondent of the *Journal* of the American Medical Association writes that on October 26 the Berliner medizinische Gesellschaft celebrated its semi-centennial. The memorial meeting was of peculiar importance, as this society is not only the largest medical association in Berlin, but also one of

the most noted and largest in Germany and can look back on a notable past. The society was formed by the union of two Berlin societies. The older of these was founded in 1844 as a society for scientific medicine and the younger was the Verein Berliner Aerzte, founded in 1858. In order to obviate any difficulties arising from the union of the two societies, which took place in 1860, Rudolf Virchow resigned his position as president of the society for scientific medicine, in favor of Albrecht v. Gräfe, the president of the other society. The Berlin Medical Society, in spite of the separation from it of numerous special associations, of which that founded by v. Leyden as the Verein für innere Medizin was the first, has still remained the central point of scientific medical activity in Berlin, and almost every Berlin physician considers it his duty to belong to it, so that at present it has more than 1,600 members. As a result of the participation of the university teachers, the scientific proceedings are always valuable and a number of important discoveries have been presented here for the first time. As successors of v. Gräfe, Bernhardt von Langenbeck, Rudolf Virchow, Ernst v. Bergmann, and finally H. Senator have held the office of president. Robert Koch, Helmholtz and Pasteur were made honorary members, a distinction which has seldom been bestowed. On the occasion of the semi-centennial the number of honorary members was considerably increased, including in addition to two living members of the society, two physicians practising in Berlin, Professor Waldeyer and the surgeon general of the Prussian army, v. Schjerning, Naunyn (Baden-Baden), Exner and Fuchs (Vienna), Golgi, Armauer-Hansen (Christiania), Abraham Jacobi (New York), Fr. Koranyi (Budapest), Keen (Philadelphia), Kitasato (Tokio), Laveran (Paris), Lépine (Lyons), Lister, Murri (Bologna), Pawlow (St. Petersburg), Ramon y Cajal (Madrid), Retzius (Stockholm), Salomonsen (Copenhagen) and Röntgen (Munich). Senator was elected honorary president. On the festival day it was announced that so far $15,000 (63,000 Marks) had been subscribed by the members for the building of the projected Virchow House, which is to be the special home of the society. The widow of Virchow on the same occasion announced that she would make over to the new building the private collections and valuable memorials of Virchow.

AT a meeting of the trustees of the Beit memorial fellowships for medical research, the reports by the fellows on their work during the past year, which had been considered by the advisory board, were approved. The following is a list of those who were elected to fellowships, the subject of research and the institution in which it is to be carried forward:

Thomas Renton Elliott, M.D. (Cantab.), M.R.C.P. (Lond.). The pathological changes in the suprarenal glands. Medical School of University College Hospital.

Eric Edwin Atkin, M.B. (Cantab.). A group of toxins with respect to the manner of destruction, mode of neutralization by antibody, and effect of the various modifications upon the animal organism. The Bacteriological Laboratory of the London Hospital.

Frances Mary Tozer, B.Sc. (Lond.). The presence of sensory fibers in the third, fourth and sixth cranial nerves; their influence upon ocular paralysis in locomotor ataxia and other diseases, and the site of the ganglion cells. The Physiological Laboratory of the Liverpool University.

Richard Williams Harold Row, B.Sc. (Lond.). The structure, development and functions of the pituitary body in vertebrata. King's College, London, the Marine Biological Association's Laboratory, and the Naples Zoological Station.

Henry Priestley, M.B., Ch.M. (Sydney). The diphtheroid organisms with regard to their distribution, morphology, cultural characteristics, pathology and relationship to diseased conditions of man and animals. The Lister Institute of Preventive Medicine.

Frederick Perera Wilson, M.D., M.Sc. (Liverpool). The changes in the lipoids of the tissues produced by syphilis and their relation to hæmolysis and immunity. The Biochemical Department of the University of Liverpool.

Arthur Gurney Yates, M.D. (Edin.). The bacteriology of acute rheumatism. The Pathological Department of the University of Sheffield.

Annie Homer, D.Sc., T.C.D. The chemistry and

physiology of tryptophane; the metabolism and chemistry of hæmoglobin in so far as they bear on its production in the animal body; the comparison of normal and pathological tissues as regards their content of intracellular ferments. The Physiological and Chemical Laboratories, Cambridge.

Frederick James Fitzmaurice Barrington, M.B., B.S. (Lond.), F.R.C.S. The functions of the male accessory genital glands. University College Medical School.

John Foster Gaskell, M.B., B.C. (Cantab.), M.R.C.P. (Lond.). The origin of the suprarenal body in the invertebrates and lower vertebrates and the function of chlorogogen cells in invertebrates. St. Bartholomew's Hospital Medical School.

UNIVERSITY AND EDUCATIONAL NEWS

Harvard University has received an additional gift of $100,000 from Mr. Adolphus Busch, of St. Louis, Mo., towards the erection and maintenance of the Germanic Museum. This sum makes a total of $350,000 given to the museum by Mr. Busch.

It is stated in the *Yale Alumni Weekly* that owing to the lack of room, notwithstanding the great wealth of material, the work of mounting prehistoric animals for public exhibition at Peabody Museum has been temporarily discontinued. The development of the resources of the museum must apparently await new building construction. This may involve an entirely new site and plant in accordance with the university development on the Hillhouse property. The building fund of the institution, according to the last report of the university treasurer, amounted to $173,923.

The Harvard University Catalogue shows this year a total enrollment of 4,123 students in the university exclusive of the summer schools, Radcliffe College, and the university extension courses. The total number of students is 77 more than it was at the corresponding period last year. The attendance in the college is 48 less than it was last year, but this decrease is more than offset by gains in the graduate and law schools.

Mr. Clarence T. Johnston has been appointed professor of civil engineering at the University of Michigan, succeeding Professor Emeritus J. B. Davis, retired on the Carnegie grant. Professor Johnson was graduated from the University of Michigan as an engineer in 1895, and received the degree of C.E. in 1899. He was state engineer of Wyoming during the period in which were formulated the irrigation laws.

At a meeting of the trustees of Princeton University on January 12, 1911, William Gillespie, assistant professor, and George David Birkhoff, preceptor, were made full professors of mathematics. Ulric Dahlgren, assistant professor of biology, was made full professor.

DISCUSSION AND CORRESPONDENCE

CARELESS CRITICISM

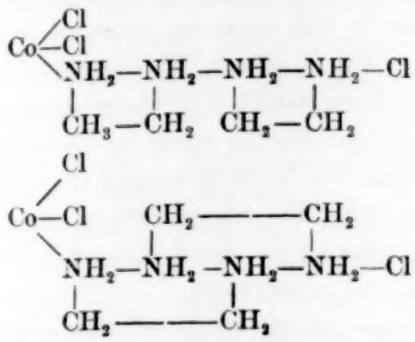
Within the past year a new book has appeared, bearing the title "Recent Advances in Physical and Inorganic Chemistry," by A. W. Stewart. The book has received very favorable comment from the reviewers in various chemical journals; and deservedly so, for the author has selected certain striking lines of advance and has pointed out the chief experimental evidence on which these are based.

There is one glaring error, however, which seems to have escaped the notice of the reviewers. In all of the chapters, except one, the author writes from the standpoint of the record as shown in chemical literature, but in this, the seventh chapter, The Cobaltammines, he departs from his usual conservatism and assumes the rôle of a caustic critic.

After a discussion of the various views put forward to explain the structure of these compounds, the author plunges into the Jörgensen-Werner controversy, defending very earnestly Jörgensen's views and criticizing with equal warmth those of Werner. On page 121, following a discussion of the points at issue between Jörgensen and Werner, the author states:

> Now, since all these difficulties arise only from the assumption that the ethylene diamine series of compounds are exactly parallel to the tetrammino-compounds, the simplest way out of the difficulty seems to be to abandon any such parallelism. Jörgensen pointed out that in the case of

ethylene diamine, we can imagine two possible arrangements with the cobaltammine molecule—



so that we might attribute the isomerism in these compounds to this difference. Now, in the ammonia compounds such an isomerism could not occur, owing to the similarity of all four ammonia groups. We should therefore expect to find no isomerism in the case of the ammonia-compounds of the type $[(NH_3)_4CoCl_2]$; *and, as a matter of fact, no such isomers are known; the compound exists in one form only.*

And again on page 125, in summarizing the whole controversy, the author states:

The question at issue is quite clear. Jörgensen points out that if we take the three cases of the dichloro-diethylene-diammino salts, the dinitrito-tetrammino salts and the dichloro-tetrammino salts, two isomeric series are known in the case of the first two sets, but the dichloro-tetrammino compounds occur in one form only—

$\left[Co\begin{matrix}Cl_2\\en_2\end{matrix}\right]X$ Occur in two forms.

$\left[Co\begin{matrix}(NO_2)_2\\(NH_3)_4\end{matrix}\right]X$ Occur in two forms.

$\left[Co\begin{matrix}Cl_2\\(NH_3)_4\end{matrix}\right]X$ Only one form known.

So that in each case where isomerism is observed there are either two nitro-groups or two ethylene diamine molecules. Where these are both absent, no isomerism occurs. Werner, on the other hand, maintains that his theory accounts better for the facts, *though he has not been able to produce the two isomeric tetrammino salts which, according to his views, ought to exist.* The non-production of these salts is specially significant when we consider how easily we can transform one di-ethylene-diamine isomer into the other; evaporation with mineral acids produces one form, from which the other can be regenerated by evaporating with water after making the solution neutral.

Briefly, Dr. Stewart contends that Werner's views are untenable as he has not been able to prepare the isomeric modification of dichloro-tetrammino cobaltic chloride. This is the crowning argument with which the chapter is closed.

Unfortunately for the argument, Werner published in 1907 in so accessible a journal as the *Berichte der Deutschen Chemischen Gesellschaft,* Vol. 40, p. 4817, a full account of the discovery, the method of preparation, and the properties of this second and isomeric modification of dichloro-tetrammino-cobaltic chloride. This was no accidental discovery, nor the result of haphazard experiment, but a logical consequence of the extension of Werner's views to the complicated poly-nucleal compounds, a field brilliantly developed by Werner during the past twelve years, but not mentioned in the chapter on Cobaltammines.

The critic can not afford to be careless in keeping up with the literature of a subject. The plea of recent publication of Werner's work on this compound can not be put forward, as Dr. Stewart has included in this chapter the still later (1908) published views of Ramsay and of Friend; indeed, the date of his preface, September, 1909, shows that the manuscript was in hand two years after Werner's announcement in the *Berichte* of his success in preparing the isomeric modification of dichloro-tetrammino-cobaltic chloride.

CHAS. H. HERTY
UNIVERSITY OF NORTH CAROLINA,
CHAPEL HILL, N. C.,
December 16, 1910

SCIENTIFIC BOOKS

The Differentiation and Specificity of Corresponding Proteins and Other Vital Substances in Relation to Biological Classification and Organic Evolution. The Crystallography of Hemoglobins. By E. T. REICHERT and A. P. BROWN. Washington, D. C., published by the Carnegie Institution of Washington. 1909.

This is an important and very interesting work, the combined production of a physiologist and a crystallographer. This review will be restricted to a consideration of some

of the results that are of interest to the biologist.[1]

Hemoglobin, the coloring matter of the blood, which in vertebrates is contained in the blood corpuscles, and which is a combination of a proteid, the globin, with the coloring matter proper, the hematin, can more or less readily be obtained in the form of crystals. Many investigators have studied the conditions under which these crystals form and some of their characters in different species of animals. The work of Reichert and Brown is, however, by far the most thorough and extensive investigation which has so far been published, and on the basis of their careful studies the authors arrive at broad biological conclusions, which will be of great interest to a wide circle of scientists.

The results obtained leave no doubt that the crystals of the hemoglobin of different species are not identical, and the work, moreover, raises problems which have not been considered by previous investigators. The authors show that not only do the crystals of different species of animals differ, but also that this difference is a graded one, corresponding to the position which the various species occupy in the zoological system. As the crystals of nearly related species resemble each other more closely than those of more distantly related species, the character of the crystals permits us to decide, within certain limits, how nearly related to each other animals are. Furthermore, inasmuch as crystallographic characters are the expression of chemical constitution, we may conclude that the hemoglobin of various species shows gradations in its chemical constitution corresponding to the close or more distant relationship of the species. We may also assume that gradations parallel to those shown by the crystals of hemoglobin exist in the other proteins. Thus we have a means of testing by crystallography the classification of animals, which at present is based almost entirely on morphological characters.

The comparative study of the crystals of hemoglobin by the authors confirms, on the whole, the correctness of the classification generally adopted by zoologists, but there are certain instances in which the authors believe that their findings suggest a revision. To cite two examples: (1) It is ordinarily assumed that the white rat is an albino of the black rat; the crystallographic examination of the various species of rats shows, however, that the white rat is closely related to the brown or Norway rat, while the black, or Alexandrine rats are more nearly related to each other. (2) The crystals of hemoglobin from the brown bat show a considerable resemblance to the crystals of hemoglobin of Papio, which belongs to the primates. This agrees with the view of some zoologists, according to which the bats are related to the primates, although, on the other hand, the crystals of the fruit bat do not show this resemblance to those of Papio.

On the whole, the main results of Reichert and Brown harmonize very well with our general biological conceptions of the graded relationship of differences in the chemical constitution of proteid substances, a conception mainly founded upon the results obtained through the application of the so-called biological tests (precipitin, anaphylaxis, complement fixation reactions), and especially are the conclusions of these authors in good agreement with the results of experiments in which watery extracts of erythrocytes have been used as an antigen and in which the erythroprecipitins obtained were shown to be specific for the antigens of the various species which had been employed. These studies have definitely proved that the proteid substances from

[1] The reviewer consulted two experts in crystallography in regard to the soundness of the crystallographic basis of the work, and he may be permitted to cite here a few sentences:

Professor Edward H. Kraus, of the University of Michigan, states: "From the crystallographic standpoint, Professor A. P. Brown has done a remarkable piece of work which is deserving of highest credit."

Dr. J. E. Pogue, of the Smithsonian Institution: "The actual crystallographic and optical details are apparently determined with skill and accuracy. The photomicrographs are excellent, and the line drawings good."

the various organs of a certain species have in common a "species" group that is characteristic for one species and differentiates it from nearly related species. By these "biological" methods it had also been found that the constitution of these groups of proteid is more similar in nearly related than in more distantly related species, and several investigators, especially Osborne and Abderhalden, had previously taken up the problem of the chemical characterization of species differences by the methods of analytical chemistry.

The work of Reichert and Brown differs, however, in some important aspects from the previous investigations (differentiation by biological tests). While in the latter the substances compared with each other were usually chemically not well defined, Reichert and Brown worked with a definite chemical substance, the hemoglobin. By the method of crystallization it is, moreover, possible to detect differences in chemical constitution which are at present not accessible to ordinary chemical analysis and in this way it is possible to differentiate between species so nearly related, that even by means of the so-called biological tests the differentiation is accomplished only with great difficulty.

After a study of Reichert and Brown's work little doubt is left in the mind of the reader in regard to the usually close relationship of the crystals of the species belonging to one genus or occasionally even of the great similarity of the crystals of two nearly related genera; it seems, however, not yet established that a *general* parallelism exists between the true relationship of the various orders and classes of animals and the character of their hemoglobin crystals.

It might furthermore be questioned in those cases in which a divergence seems to exist between the ordinary classification and the results of the crystallographic studies, whether the latter should be accepted unhesitatingly. It rather seems that in such cases of doubt a comparative study by the crystallographic, and by the so-called biological tests should be used to confirm the former. This would be especially desirable in view of the great lability of the hemoglobin molecule which the authors themselves repeatedly emphasize. We might, *e. g.*, question whether the different kinds of crystals of oxyhemoglobin found occasionally in the same blood might not be the result of certain secondary chemical changes in the hemoglobin molecule. In this connection it is of interest that in a recent publication Offringa states, that if the crystals of hemoglobin of the horse are prepared without the addition of any salt (Reichert and Brown made use of oxalates in their work), only one kind of crystals is obtained.

In the work of Reichert and Brown, as well as in the more recent literature generally, the term "specific" is frequently encountered, and it is apparently supposed to have a definite biochemical meaning. A more searching analysis, however, reveals the fact that this term includes three different relationships between substances, and a clear distinction between these appears to be very desirable. Several years ago the reviewer described a class of substances which he designated as specifically adapted; these are substances coexisting in the same organism and showing functional relations to each other. In other cases the term "specific" merely indicates a character of a certain species by which it differs from other species, without any parallelism existing between this characteristic and the zoological classification. In a third class of specific characters such a relationship exists, and this third kind of specificity might appropriately be called "generic specificity." With the latter, we have principally to deal in the work of Reichert and Brown.

The work of these authors includes also a consideration of the general characters of hemoglobins as well as a summary of other differences which have been found in the blood of different species by previous investigators. With some opinions expressed in this part of the book issue might, perhaps, be taken, as for instance, in regard to the low estimate of the physiological importance of iron as a constituent of hemoglobin; a view

against which especially the recent investigations of W. Manchot might be cited.

The fine reproduction of 100 plates of beautiful microphotographs of hemoglobin crystals of various species of animals is especially noteworthy.

Leo Loeb

A Monograph of the Culicidæ or Mosquitoes. Volume 5. By Fred V. Theobald. British Museum (Natural History). London. 1910. Pp. xvi + 646. 261 text figures, 6 plates.

The author has assembled in this volume descriptions of many recently erected genera and species, characterized thirteen genera and eighty species as new, supplied keys for the separation of the genera and a very large proportion of the described species and, in addition, gives observations or references to practically all other genera and species. This latter makes the fifth volume practically a systematic index to the preceding four volumes and will greatly facilitate future studies in this group.

The modified classification proposed by Lutz and outlined in volume four has been closely followed. We regret to note that the tables for the recognition of the genera are based largely upon scale structure, a method of separation which has found comparatively slight favor in America, though we can not ignore the author's statement, especially in view of his wide experience with these insects from all parts of the world, that separation in this manner is comparatively easy, as evidenced by the number of correctly named collections received at the British Museum. The monograph, as a whole, is weak from a structural and biological standpoint, and necessarily so in many instances, especially in the case of forms received from distant countries where methods of collection and preservation are far from ideal. The study of the imago must, as a rule, precede biological investigations, and it is therefore not surprising that the immature stages have received comparatively little attention in this work. We sympathize strongly with the author in his declining to recognize genera and species based solely upon larval characters, despite the fact that such procedure is not sanctioned by the International Code of Zoological Nomenclature. It is true that good characters are found in the larvæ of this group, and that in some instances species are more readily separated in the larval than in the adult form. Nevertheless, our classification of the family is based upon the imago, and confusion is bound to result from the employment of a double standard; though technically allowable, it is in this group questionable procedure.

There are important problems in synonymy which should be settled in the near future for the purpose of avoiding confusion if for no other reason. The author declines to accept the broad delimitation of Aedes, recently proposed in this country, and, as a consequence, the nomenclature used by a number of American workers differs widely from that employed in the volume under consideration. Personally, without having made special study of the problems involved, we question the wisdom of attempting to unite under one name such diversified forms. On the other hand, a number of generic names have been allowed to stand as valid in this work which will probably fall as synonyms because of the absence of satisfactory characters. These and similar questions can be settled only by an exhaustive comparative study of the characters presented by the immature stages as well as those of the adult. The key to the solution of many of these problems will be found in the unrivaled collections from all parts of the world, now assembled in the British Museum of Natural History.

The diversity and size of this group is indicated by the eight subfamilies recognized (excluding the Corethridæ), comprising some 146 genera and 899 species, a large majority of these being valid. The world owes Professor Theobald a debt of gratitude for assembling, carefully describing and arranging this immense amount of material, among which are included some of the most dangerous insect enemies of man. Prior to this study, our knowledge of the Culicidæ was

little better than chaotic, many species being unknown or else grouped under a specific name mostly noteworthy because of its comprehensiveness. We think all will agree that Professor Theobald has done a large amount of valuable pioneer work, though we may not adopt all of his taxonomic views. The British Museum is to be congratulated upon having published such an admirable work, of which the volume under consideration forms only a part, consisting of five good-sized volumes, illustrated by a large series of figures, there being over 1,200 text illustrations and 88 magnificent plates, and characterizing practically all the known species in this important family. It is perhaps needless to add that this monograph on the Culicidæ, possibly not even yet completed, must be the major foundation for all subsequent studies in this group and therefore nearly indispensable to the systematist.

E. P. FELT

SPECIAL ARTICLES

NEW PHENOMENA OF ELECTRICAL DISCHARGE

AT a meeting of the Academy of Science of St. Louis on December 5, the writer gave further results of work on electrical discharge. It had been previously shown that oscillations of widely varying frequency, attended by musical tones, could be brought about, by means of small spark gaps of variable length, in the lines leading from the terminals of an influence machine to the main gap across which the discharge is to pass. In a former paper it had been suggested that the striæ in a vacuum tube were in the nature of waves in an organ pipe.

These results suggested the idea of imposing resonance vibrations in a column of air contained in a glass tube, which also contained terminals from the influence machine. The air vibrations were produced by means of a blast of air from a pressure tank, which was directed across the mouth of another tube. It has been found that with very careful adjustments, the electric discharge across a small gap in the glass tube could be affected in a marked way by the impressed sound waves. A luminous discharge was apparently converted into a dark discharge.

The line within the tube containing this gap was in multiple with another line containing an adjustable gap. This system was in the positive side of a circuit which contained a long discharge gap. Placed transversely in this gap was an insulated sheet of copper, which served to prevent disruptive discharges. Attempts are now being made to cause an organ pipe to sound a musical tone by means of periodically varying electrical stresses within the air-column. The response of the air-column is not as marked as when the vibrations are produced in the ordinary way. Some effects have however been secured, and there is every reason to believe that the attempt will meet with success. These results can only be considered as preliminary.

Several friends have suggested that the term drainage column as applied to the positive or luminous end of a discharge, was another name for an ionized mass of air. If we say that air is ionized by X-rays passing through it, this term does not represent the conditions at either terminal of an influence machine.

In a mass of air ionized by X-rays, the average charge of a molecule is the normal charge. Those which have a greater than normal charge mingle with those which have a less than normal charge. Such a mass of air will respond to the demands of an electrometer placed within it, whether its leaves have a greater or a less than normal charge. The supercharged molecules will deliver the excess to those whom they have robbed, or to any others which may be in a like condition. A similar statement may be made concerning the molecules which have less than the normal charge. But such a mass of air is not a drainage column. It is in a condition which promotes the formation of a drainage column, if the terminals of some "source" of electricity like an influence machine are placed within the ionized mass of air. This mass of air is then made a part of the conducting circuit, by the starting of the machine into action. The fact that it behaves differently from the rest of the circuit is incidental to the fact

that it is in gaseous condition. The apparent discharge into the air from the positive terminal was shown by phenomena described in my paper of February 18, 1910,[1] to be an inflow of negative electricity to that terminal. The electric fluid is thus drained from a column of air which begins at the positive terminal and extends towards the negative or compression terminal. After this drainage has been brought about, the air within this column is in a very different condition from that of air ionized by X-rays. The average charge per molecule of air is then less than the normal amount. By placing an insulated sheet of copper between the terminals, the drainage or conduction column is prevented from reaching the mass of supercharged air in front of the negative terminal. Loud disruptive discharges will pass when the plate is removed, and they cease at once when the plate is placed midway between the terminals. On moving the copper plate nearly to the negative glow, the drainage column follows it, and a torrent of sparks will pass.

The so-called positive ions do not emerge from the wire at the positive terminal, and they do not enter the wire at the negative terminal. When this air is nearly all removed from the discharge gap, as is done in the Crookes tube, the cathode discharge still continues. The drainage column has disappeared at the anode. When the air is partially removed, thus increasing the mean free path, the drainage column may have a great length, and it follows all of the windings and bends of the tube. It would not do this if it were an outward discharge like that from the negative terminal.

This drainage column constitutes the canal rays, in a tube where the gas has been partly exhausted, so that the mean free path has been increased. In a paper to be published by the Academy of Science of St. Louis, phenomena of canal rays in air of ordinary pressure will be presented, in connection with phenomena involved in vibrations imposed upon the air column.

[1] *Trans. Acad. of Sc. of St. Louis*, Vol. XIX., No. 1, Plates II. to VIII.

If any trace of a positive fluid capable of moving through a wire as an electric current must move could be found, and if the two-fluid theory could furnish an adequate and rational explanation of these and other phenomena, there would be no objection to its use in an exposition of the subject, as has been the custom heretofore.

In 1895 the writer showed that the velocity of flow of the electric fluid in a pumping service then discussed, must be very great. Imagine two spheres having radii equal to that of the earth. Suppose electricity to be pumped from one into the other, until their difference in potential is 50 volts. Connect them by means of a 50-volt one-ampere lamp. It would begin to glow with normal brightness. In order to keep the voltage constant the radii of the two spheres must be diminished with a uniform velocity of more than half the velocity of light. This store of electricity would maintain this lamp at normal candle power for 0.035 second. The operation must be repeated 28 times a second. The time for one stroke of the piston is in seconds, $t = Rr$, where r is the radius of the spheres in cm. and R is the resistance of the lamp in electrostatic units. If the sphere from which the electric fluid were pumped, by some adequate means, had an infinite capacity, the other sphere must then be charged to a potential of 50 volts, and the above results would remain unchanged. The surface of the charged sphere would then be the piston of an electrical pump, and its velocity would be as given above. All of this store of electricity must pass through the lamp in 0.035 second.

We may learn much about the constitution of matter by a study of what are called the positive ions when an electric discharge is passed through a column of gas. But nothing has yet been learned to indicate that these ions play any part in a lamp circuit, except that they then constitute the solid conductor.

Wheatstone's work now shows us that when half a mile of copper wire is placed centrally in the spark gap, we have compression and rarefaction waves in Franklin's fluid, which meet at or near the middle of the wire. The

positive ions are in some way linked together, and transmit Thomson's corpuscles with immense velocity. There is a certain amount of shaking up involved in this transfer. It is the Joule effect. The positive ions remain at rest, and there are, therefore, no canal rays. When this wire is removed from the spark gap, the gas molecules receive the same compression and rarefaction waves, if the man who turns the crank of the machine continues his work. At the negative terminal the air molecules are loaded with the corpuscles, in the region of negative glow. They are then urged by convection as carriers, across the Faraday dark space. At the positive terminal the corpuscles pass from the gas into the metal conductor by a rarefaction or drainage process. Photographic plates reproduced in former papers[2] show that the drainage lines begin at the positive terminal. In this drainage column the carriers of the discharge move in a direction opposite to that in which the discharge is being urged. Cakes of ice floating on water would behave in a similar way, if a runner should jump from one to another, although the mechanism would be different. Nevertheless, such behavior of cakes of ice appears to be related to the athletics of the foot race, in somewhat the same way that positive ions in a gas are related to the flow of electricity in a power circuit.

FRANCIS E. NIPHER

JOINT MEETING OF MATHEMATICIANS AND ENGINEERS AT MINNEAPOLIS

THREE years ago in connection with the convocation of the American Association for the Advancement of Science in Chicago, a joint meeting of mathematicians and engineers was arranged through a committee of the Chicago Section of the American Mathematical Society. This meeting aroused much interest and resulted in the appointment of a committee of twenty, under the chairmanship of Professor E. V. Huntington, of Harvard University, to consider the whole question of the teaching of mathematics to students of engineering in this country, and to report its recommendations to the Society for the Promotion of Engineering Education at its summer meeting to be held at Madison, Wis., in June, 1910. This committee was constituted as follows: Philip R. Alger, professor of mathematics, U. S. Navy, Annapolis, Md.; Donald F. Campbell, professor of mathematics, Armour Institute of Technology, Chicago, Ill.; Edmund A. Engler, president of the Worcester Polytechnic Institute, Worcester, Mass.; Charles N. Haskins, assistant professor of mathematics, Dartmouth College, Hanover, N. H.; Charles S. Howe, president, Case School of Applied Science, Cleveland, Ohio; Emil Kuichling, consulting civil engineer, New York City; William T. Magruder, professor of mechanical engineering, Ohio State University, Columbus, Ohio; Ralph Modjeski, civil engineer, Chicago, Ill.; William F. Osgood, professor of mathematics, Harvard University, Cambridge, Mass.; Charles S. Slichter, consulting engineer of the U. S. Reclamation Service, professor of applied mathematics, University of Wisconsin, Madison, Wis.; Charles P. Steinmetz, consulting engineer of the General Electric Company, professor of electrical engineering, Union University, Schenectady, N. Y.; George F. Swain, consulting engineer, professor of civil engineering, Harvard University, Cambridge, Mass.; Edgar J. Townsend, dean of the College of Science and professor of mathematics, University of Illinois, Urbana, Ill.; Frederick E. Turneaure, dean of the College of Mechanics and Engineering, University of Wisconsin, Madison, Wis.; Clarence A. Waldo, head professor of mathematics, Washington University, St. Louis, Mo.; Gardner S. Williams, consulting engineer, professor of civil, hydraulic and sanitary engineering, University of Michigan, Ann Arbor, Mich.; Calvin M. Woodward, dean of the School of Engineering and Architecture and professor of mathematics and applied mechanics, Washington University, St. Louis, Mo.; Robert S. Woodward, president of the Carnegie Institution of Washington, Washington, D. C.; Alexander Ziwet, professor of mathematics, University of Michigan, Ann Arbor, Mich.

In the early part of its investigation the committee collected a large amount of information in regard to the present status of mathematical instruction for engineering students. Since that time, however, a much more inclusive inquiry has been undertaken by the International Commission on the Teaching of Mathematics, of which the American Commissioners are Professors D. E. Smith, J. W. A. Young and W. F. Osgood. In

[2] *Trans. Acad. of Sc. of St. Louis*, XIX., Nos. 1 and 4, Plates X., B, XX., A, B and C, and XXI., A.

order to avoid unnecessary duplication, this committee voted to turn over all the results of its own inquiry in this field to the larger commission, to be worked up in accordance with the general scheme adopted by that commission, and to be incorporated in their report. This material is therefore not included in the present report.

Aside from the collection of data, Professor Huntington's committee decided that the most important need at the present time is a series of synoptical text-books, which shall present: (1) the fundamental principles of the science, in compact form, and (2) a classified and graded collection of problems (which would naturally be subject to continual change and expansion). It is their hope that the Minneapolis report, which is confined to the first part of the desired text-book, will stimulate throughout the country practical contributions toward the second.

Copies of these syllabi were distributed among the hundred or more engineers, physicists and mathematicians who joined in a good fellowship dinner on the evening preceding the presentation of the report. Additional copies may be had upon request from the chairman. The following quotations from the preface will indicate its scope and purpose:

"The object of this report is to present a synopsis of those fundamental principles and methods of mathematics which, in the opinion of the committee, should constitute the minimum mathematical equipment of the student of engineering.

"It is hoped that this report may be serviceable in two ways: first, to the teacher, as an indication of where the emphasis should be laid; and secondly, to the student, as a syllabus of facts and methods which are to be his working tools. It does not include data for which the student would properly refer to an engineer's hand-book; it includes rather just those things for which he ought never to be obliged to refer to any book—the things which he should have constantly at his fingers' ends.

"The teacher of mathematics should see to it that at least these facts are perfectly familiar to all his students, so that the professor of engineering may presuppose, with confidence, at least this much mathematical knowledge on the part of his students. On the other hand, if the professor of engineering needs to use, at any point, more advanced mathematical methods than those here mentioned, he should be careful to explain them to his class.

"The defects in the mathematical training of the student of engineering appear to be largely in knowledge and grasp of fundamental principles, and the constant effort of the teacher should be to ground the student thoroughly in these fundamentals, which are too often lost sight of in a mass of details.

"The committee has not found it possible to propose a detailed course of study. The order in which these topics should be taken up must be left largely to the discretion of the individual teacher. The committee is firmly of the opinion, however, that whatever order is adopted, the principal part of the course should be problems worked by the students, and that all these problems should be solved on the basis of a small number of fundamental principles and methods, such as are here suggested."

The report was freely discussed and frankly criticized by a large number of speakers, including the following: Professors E. J. Wilczynski, University of Chicago; A. G. Hall, University of Michigan; E. H. Comstock, School of Mines of the University of Minnesota; G. A. Miller, University of Illinois; A. E. Haynes, College of Engineering of the University of Minnesota; E. F. Nichols, president of Dartmouth College; T. F. Holgate, dean of Northwestern University; Alexander Ziwet, University of Michigan; E. R. Maurer, College of Engineering of the University of Wisconsin; Henry Crew, department of physics of Northwestern University; H. E. Slaught, University of Chicago; B. L. Newkirk, University of Minnesota; E. F. Chandler, University of North Dakota; J. J. Flather, head of mechanical engineering, University of Minnesota, and L. E. Dickson, University of Chicago, chairman of the meeting. Also numerous discussions in writing were received by the chairman or through the secretary, including one from President R. S. Woodward, of the Carnegie Institution of Washington, one from Professor A. E. Haynes, of the University of Minnesota, and one from William Kent, president of the Technical Literature Company of New York.

The discussion finally led to the following resolution, which was unanimously adopted:

Resolved, that this body tenders hearty thanks to Professor Huntington for the great interest which he has shown in this work and the untiring service which he has given to it; that we commend the work of the entire committee for the preparation of a report which it is believed must operate for betterment along the lines of its

recommendations; and while not prepared to approve in all respects the details, especially in the syllabus on dynamics, as shown by the full and free discussion at this meeting, yet we heartily endorse the spirit of the report and thank the officers of the Society for the Promotion of Engineering Education, who have shown their friendly cooperation in offering to publish these syllabi in their official *Bulletin*, for the purpose of drawing out further criticisms and suggestions either in printed papers or in written communications to the chairman of the committee.

The further report of this committee is to be presented at the next meeting of the Society for the Promotion of Engineering Education in June, 1911. The present meeting was organized, as was the original meeting in Chicago in 1907, by the Chicago Section of the American Mathematical Society.

Other papers read before the Chicago Section at the Minneapolis meetings, aside from those in astronomy for which joint sessions were held with Section A, were as follows:

"On the Use of the Co-sets of a Group," Professor G. A. Miller, University of Illinois.

"Congruencial Theory of Functions of Several Variables," Professor L. E. Dickson, University of Chicago.

"Generalizations of Theorems on Linear Algebras," Professor L. E. Dickson.

"On Primitive Roots of Ideals," Professor Jacob Westlund, Purdue University.

"The Problem of Defining the Set of Real Numbers," Dr. A. B. Frizell, University of Kansas.

"A Historical Note on the Newton-Raphson Method of Approximations," Professor Florian Cajori, Colorado College.

"One Parameter Families and Nets of Plane Curves," Professor E. J. Wilczynski, University of Chicago.

"Circular Numbers for a Plane Curve," Dr. Horace T. Burgess, University of Wisconsin.

"Rational Anharmonic Curves upon a Quadric," Dr. Horace T. Burgess.

"The Applications of Matrices to Cubic Forms," Dr. Horace T. Burgess.

"Envelopes of One Parameter Families of Plane Curves," Professor Walter J. Risley, James Millikin University, and Professor W. E. MacDonald, Canton College, China.

"A Reduction of Two Power Series in Many Variables to Two Equivalent Polynomials," Dr. W. D. MacMillan, University of Chicago.

"The Path of Light in a Medium Homogeneous in Concentric Spherical Layers," Dr. Harris F. MacNeish, Yale University.

"The Curves of Equal Action for Elliptical Coordinates," Professor Kurt Laves, University of Chicago.

"Robert of Chester's Translation of the Algebra of Al-Khowarazmi," Professor L. C. Karpinski, University of Michigan.

"Hindu Numerals in the Kitab al Fihrist," Professor L. C. Karpinski.

"Ruled Surfaces and Planed Hypersurfaces in Four-Dimensional Space," Dr. Arthur Ranum, Cornell University.

"Transformation Groups and Substitutions of an Infinite Degree," Dr. L. I. Neikirk, University of Illinois.

"Use of Quaternions in Differential Geometry," Professor J. B. Shaw, University of Illinois.

"On Plane Quintic Curves," Dr. H. L. Slobin, University of Minnesota.

"On the Construction of a Certain Class of Periodic Solutions of the Problem of Three Bodies," Professor F. R. Moulton, University of Chicago.

"Curves on Quintic Scrolls," Professor Frank B. Williams, Clark College.

The next meeting of the Chicago Section will be in Chicago on Friday and Saturday, April 28 and 29, 1911.

H. E. SLAUGHT,
Secretary of the Section

THE AMERICAN PHYTOPATHOLOGICAL SOCIETY

THE second annual meeting of the American Phytopathological Society was held in affiliation with the American Association for the Advancement of Science at Minneapolis, Minn., December 28–30, 1910. The attendance was not as large as at the Boston meeting, but an excellent program of twenty-eight papers was presented. A joint session with Section G of the American Association and the Botanical Society of America was held at the Minnesota Agricultural College on Thursday, December 29, and other sessions at the University.

The society wishes to express its appreciation and gratitude to the local committee and especially to Dr. F. E. Clements for the excellent rooms and facilities provided.

The constitution of the society, drafted and approved by the council, was adopted. The following officers were elected:

President—Professor A. D. Selby, Ohio Agricultural Experiment Station, Wooster, Ohio.

Vice-president—Dr. R. A. Harper, University of Wisconsin, Madison, Wis.

Secretary-treasurer—Dr. C. L. Shear, U. S. Department of Agriculture.

Members of Council—Dr. G. P. Clinton, Connecticut Agricultural Experiment Station, New Haven, Conn., one year; Dr. Erwin F. Smith, U. S. Department of Agriculture, two years. The two *ex officio* members of the council are Dr. F. L. Stevens, retiring president, and Dr. L. R. Jones, chairman of board of editors of *Phytopathology*.

The following editorial board for the new journal *Phytopathology* was elected:

Editors—L. R. Jones, C. L. Shear, H. H. Whetzel.

Associate Editors—G. P. Clinton, E. M. Freeman, H. T. Gussow, F. D. Heald, Haven Metcalf, W. A. Orton, W. M. Scott, A. D. Selby, Erwin F. Smith, Ralph E. Smith, F. L. Stevens and Roland Thaxter, with Donald Reddick as business manager.

The society elected thirty-seven new members.

The next annual meeting will be held at Washington, D. C., in affiliation with the American Association for the Advancement of Science.

The following committee on legislation concerning plant diseases was appointed: Mel T. Cook, F. L. Stevens, F. C. Stewart, H. T. Gussow and A. D. Selby, *ex officio*. The committee on names of plant diseases presented a report and was continued.

The following papers were presented:

"Tannin in Relation to the Enzymes of the Plant Cell," Mel T. Cook.

"The Persistence of *Bacillus amylovorus* in Pruned Apple Twigs," H. R. Fulton. (Read by C. L. Shear.)

"The Industrial Fellowship in Plant Pathology," H. H. Whetzel.

"*Sclerotium rhizoides* Auersw. on *Calamagrostis*," A. B. Stout.

"The Relation of Temperature to Spore Germination and Infection with *Cystopus*," I. E. Melhus.

"The Rusts of White and Red Clover," Frank D. Kern. (Read by title.)

"Black Leg or *Phoma* Wilt of Cabbage, a New Trouble to the United States caused by *Phoma oleracea* Sacc," Thos. F. Manns. (Read by A. D. Selby.)

"The *Fusarium* Blight and Rot of Potato; Field Experiments in Prevention," Thos. F. Manns. (Read by A. D. Selby.)

"The White Pine Blister Rust," Perley Spaulding. (Read by Haven Metcalf.)

"A Study of some Anthracnoses and their Relations to a Sweet Pea Disease," J. J. Taubenhaus. (Read by Mel T. Cook).

"The Ascogenous Form of the Fungus causing Dead-arm of the Grape," C. L. Shear.

"Further Notes on the Bark Disease of the Chestnut," Haven Metcalf and J. Franklin Collins.

"Necrosis and Crown Gall Diseases of Grapes in New York," D. Reddick.

"Peach Leaf-curl in Nursery Stock," V. B. Stewart. (Read by D. Reddick.)

"Some Laboratory and Field Studies on Fungicidal Values," E. Wallace, F. M. Blodgett and Lex R. Hesler. (Read by H. H. Whetzel.)

"Varietal Resistance of Beans to Anthracnose," M. F. Barrus. (Read by H. H. Whetzel.)

"A Preliminary Report upon the Effects of Arsenical Compounds upon Apple Trees," B. D. Swingle. (Read by C. L. Shear.)

"Results of Cotton Anthracnose Work for 1910," H. W. Barre.

"Notes on some Diseases of Trees in our National Forests, II.," George Grant Hedgcock. (Read by title.)

"A New Fruit Spot of Apple," W. M. Scott. (Read by C. L. Shear.)

"Contribution to our Knowledge of the Mine Fungi of the United States," C. J. Humphrey.

"A Bacterial Disease of Bananas and Plantains," James Birch Rorer. (Read by Erwin F. Smith.)

"The Leaf Spot of Sugar Beets," C. O. Townsend. (Read by title.)

"Notes on Hypertrophied Structures," Mel T. Cook.

"The Grand Rapids Tomato Disease," Erwin F. Smith.

"Crown Gall of Plants," Erwin F. Smith.

"Cereal Smut Spore Germination," E. C. Stakman. (Read by title.)

"Alfalfa Rust and Timothy Rust in Iowa," L. H. Pammel.

Abstracts of these papers will appear in the April number of *Phytopathology*, the new journal of the society, and some of the papers will be published later in the same journal.

C. L. Shear,
Secretary

SOCIETIES AND ACADEMIES

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF BIOLOGY

A REGULAR meeting of the section of biology was held at the American Museum of Natural History, November 14, 1910. In the absence of Chairman Charles B. Davenport, Dr. Alexander Petrunkevitch presided. The following papers were read:

The Effects of Exposure on the Gill Filaments of Fishes: RAYMOND C. OSBURN.

Salmonoid fishes hatched and reared under artificial conditions frequently show a malformation of one or both of the gill covers, as a result of which the gill filaments are exposed. This condition has been observed in many hatcheries, the percentages sometimes being as high as twenty per cent. The deformity in the fishes studied is produced by the rolling in of the operculum. 486 yearling silver salmon reared in the New York Aquarium were examined with the following results: normal, 397; right opercle short, 44; left opercle short, 27; both opercles short, 18; percentage of abnormality, 18.31.

On examination under the microscope the exposed filaments are found to be quite abnormal. The epithelium, instead of being composed of thin flattened cells, is greatly thickened, consisting of cuboid or columnar cells, and in some cases several layers of the cells are found. The secondary laminæ, in which respiration for the most part takes place, are often reduced or wanting and the blood capillaries are not fully developed.

The hypertrophy of the epithelium, while it undoubtedly protects the filaments against abrasion, must at the same time seriously interfere with their function in respiration. The cause of the deformity of the opercle is unknown. Fish culturists have noted its appearance very early in fry, but whether it is congenital or is induced by crowding or by other untoward conditions in the hatching trays, further observations must decide.

Courtship in Tarantulas: ALEXANDER PETRUNKEVITCH.

The instincts of the male tarantula change suddenly at the period of maturity. From a creature with domestic habits he develops into a vagabond. Disregarding personal danger he constructs a sperm-web into which he throws out his sperm and pumps it then into both of his palpi. In the search for the female he is entirely dependent upon his sense of touch, his sense of sight being entirely inadequate for the purpose. The courtship is therefore very short and consists in beating the female with his front legs. The danger of being hit by the fangs of the excited female is prevented by catching them with the hooks on the front legs. The coitus lasts not longer than one half minute, after which the spiders cautiously separate. A few weeks later the males die apparently a natural death.

AT the regular meeting of this section held at the American Museum, December 12, 1910, Mr. Roy W. Miner presiding, the following papers were read:

The Effect of Changes in Water Density on the Blood of Fishes: G. G. SCOTT.

When salt water fishes are placed in fresh water they gain in weight. Investigation of the blood shows that there is a decrease in number of corpuscles per cubic millimeter and that the specific gravity of the blood decreases.

Tests with the Beckmann apparatus show that the freezing point of the blood of such fishes is higher than that of normal blood. If the fishes are placed in a solution of sea water plus sea salt, the corpuscle count is increased, the specific gravity of the blood is greater and the freezing point of the blood is depressed. A chemical examination of the chlorides of the blood of normal fish as compared with the chlorides of fishes kept in fresh water, shows that the loss of chlorides in case of the fishes experimented on is greater than the mere dilution of the blood by the endosmosis of water would account for. Hence, under the abnormal conditions to which the fish is subjected the gills become permeable to salts. The osmotic pressure of the blood is thus profoundly changed. That these changes reach the tissues is indicated by investigations now going on. The death of the fish which usually accompanies such sudden transitions as are employed in these experiments is possibly caused by conditions set up similar to those in such diseases as dropsy. It is hoped that further investigations now being carried on will clear up this question.

Marine Ecology and its Representation in a Museum: ROY W. MINER.

The speaker described the chief associations of marine animals to be found between the tides or just below the lower tide limits along the north Atlantic coast, with especial reference to the annulates and molluscs and the fauna of wharf piles in the Woods Hole region and the north shore of Long Island. The methods of collecting were then briefly outlined, and the chief steps for preserving data, observations, etc., for museum ecological groups were mentioned. The speaker then discussed the problems connected with con-

structing and installing groups and models of invertebrates in a museum. Colored slides were shown both of the living invertebrates and of their habitats, and also of the models and groups in course of construction and as completed at the American Museum. The speaker concluded by exhibiting two sketch-models prepared under his direction by Messrs. Matausch and Shimotori, of the museum staff, as preliminary studies for the annulate and pile fauna groups which are in course of construction in the American Museum.

Exhibition of Models of Membracidæ: Ignaz Matausch.

The speaker exhibited a series of six enlarged models in wax which he had prepared for the American Museum of Natural History, as well as a series of twenty-three colored drawings and a collection of typical specimens which had been sent him by Professor F. Silvestri, of Portici, Italy.

The Membracidæ, or tree-hoppers, are among the most interesting of insects. Very little is yet known concerning their life histories, a subject to which the speaker said he had devoted considerable attention. They are remarkable for their extraordinary variation in the form of the prothorax. In order to make an enlarged model it is necessary to dismember the insect and to prepare drawings of the different parts to a selected scale. The separate parts are then copied in clay; plaster molds are then prepared and casts made in wax. These are then finished, the details put in, and the whole put together and colored.

L. Hussakof,
Secretary

American Museum of Natural History

THE AMERICAN CHEMICAL SOCIETY

NEW YORK SECTION

The fourth regular meeting of the session of 1910–11 was held at the Chemists' Club on January 6.

The Nichols medal, awarded annually for the best paper read before the section, was presented to M. A. Rosanoff and C. W. Easley for their paper "On the Partial Vapor Pressures of Binary Mixtures." Professor W. D. Bancroft, president of the society, made the presentation and Professor Rosanoff accepted the medal on behalf of himself and Professor Easley.

In receiving the medal, Professor Rosanoff said:

"*Mr. Chairman, Dr. Bancroft, Dr. Nichols, Gentlemen:* I thank you all, on behalf of Dr. Easley and myself, for the great honor you are this evening bestowing upon us. It will be an ever-present support against discouragement in the research struggle to which, I hope and believe, my life will be devoted to the end.

"Much of the honor adjudged to us is due—it is a pleasure to admit—to Clark University, whose unexampled freedom, and liberalism, and Nietzschean belief in the future, form an ideal atmosphere for scientific research.

"Gentlemen, Ostwald prophesies that the center of gravity of science is bound to move from Europe across the Atlantic. This chemical society, a body devoted mainly to the furthering and encouragement of chemical research, can do more than any other group of men, more even than the universities, toward hastening that migration, which will open a remarkable era in American history. Every honor conferred by the society on the basis of research must stimulate that migration and will, therefore, rebound to the society itself."

Mr. T. J. Parker gave an account of the Minneapolis meeting of the American Chemical Society.

Dr. W. C. Moore read a paper by Professor H. N. McCoy and himself on "Organic Amalgams."

Professor Robert Kennedy Duncan addressed the section "On the Relation between Chemistry and Industry in America." Formal discussion of the subject followed by M. C. Whitaker, L. H. Baekeland and Allen Rogers.

C. M. Joyce,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 476th regular and 31st annual meeting was held at the hall of the Cosmos Club, December 10, 1910, with President T. S. Palmer in the chair.

Reports of the secretary and treasurer were read and approved. Four new members were elected.

The following officers for 1911 were elected:

President—David White.

Vice-presidents—W. P. Hay, E. W. Nelson, J. N. Rose and Edw. L. Greene.

Recording Secretary—D. E. Lantz.

Corresponding Secretary—N. Hollister.

Treasurer—J. W. Gidley.

Members of the Council—A. D. Hopkins, A. K. Fisher, Vernon Bailey, A. B. Baker and Paul Bartsch.

D. E. Lantz,
Secretary